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SPECTRAL HUE AS A FUNCTION OF INTENSITY

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It is sometimes taken for granted that each particular wavelength of monochromatic light produces one definite invariable quality of color sensation. This is far from being strictly true; as the intensity of a spectral stimulus is changed the color sensation is in general altered both in hue and saturation. In a previous paper[1] I have discussed the variations of spectral saturation with light intensity; in the present one I shall describe some observations regarding the variations of hue.

Helmholtz[2] noted that as the intensity of the spectral colors is made very great they undergo a change of hue, the colors corresponding to the longer wave lengths (red and orange) tending towards yellow, and those corresponding to the shorter wave lengths (green, blue-green and violet) tending towards blue. At the same time the colors become more whitish (less saturated), and eventually, when the intensity becomes sufficiently great, the entire spectrum becomes colorless.

It was observed by Von Bezold,[3] by Peirce,[4] and by Brücke[5] that changes of hue also occur when the intensity is made very low. These changes are often

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[1]On the saturations and chromatic thresholds of the spectral colors, *Brit. J. Psychol.*, 21, 1931, 283.

[2]H. von Helmholtz, *Treatise on Physiological Optics* (Eng. trans.), II, 1924, 183 ff.

[3]W. von Bezold, Über das Gesetz der Farbenmischung und die physiologischen Grundfarben, *Pogg. Ann. d. Phys. u. Chem.*, 150, 1873, 221.

[4]C. S. Peirce, Note on the sensation of color, *Amer. J. Sci.*, Ser. 3, 13, 1877, 247.

[5]E. Brücke, Über einige Empfindungen im Gebiete des Sehnerven, *Sitzber. d. Akad. d. Wiss. zu Wien, math.-naturw. Kl.*, 77(3), 1878, 39. See also: A. Chodin, Über die Abhängigkeit der Farbenempfindungen von der Lichtstärke, *Sammlung physiol. Abhandlungen*, herausg. von W. Preyer, Reihe 1, Heft 7, 1877; O. N. Rood, *Modern Chromatics*, 1879, chap. 7; E. Hering, Zur Erklärung der Farbenblindheit, *Lotos*, N.F., 1, 1880, 9; E. Albert, Über die Änderung des Farbentones von Spectralfarben und Pigmenten bei abnehmender Lichtstärke, *Wied. Ann. d. Phys. u.*

referred to as the "Bezold-Brücke phenomenon." A distinction between the effect of lowering intensity and that of raising intensity is, however, somewhat artificial, since there is a continuous series of hue changes over the entire intensity range.

While an increase of intensity produces a trend towards yellow or blue, a decrease of intensity alters the spectral hues, according to their wave-lengths, in the direction of red, green or violet. The orange hues become more reddish, the yellowish and bluish greens become more greenish, and the blues tend towards violet, that is, become more reddish. Von Bezold, observing the changing appearance of a spectrum as the light was diminished, noted a progressive shrinkage of the yellow and blue regions, and an expansion of the red and violet to fill their place. Eventually, according to him, there are only three colors left in the spectrum, namely red, green, and violet. He regarded the peculiar stability of these three colors as furnishing strong support for the trichromatic theory of color vision.

In 1892 König and Dieterici[6] made a precise determination of the three "fundamental sensation curves" of the trichromatic theory. These curves, as is well known, can be plotted in a great variety of ways, their form depending upon which particular red, green and blue (or violet) one chooses as "fundamental sensations" (*Grundempfindungen*). This choice is, to begin with, arbitrary. But by obtaining color-mixture data for color-blind as well as for normal observers, and by utilizing the condition that the sensation curves of the color-blind are identical with two of the three sensation curves of the normal individual, König and Dieterici were able to give a more or less exact specification of the 'true' *Grundempfindungen*. Their results were: (1) red—a purplish red outside the spectrum, complementary to 490—500 mμ; (2) green—a hue matching that of 505 mμ; (3) blue—a hue matching that of 470 mμ.

Chem., 16, 1882, 129; H. von Helmholtz, Kürzeste Linien im Farbensystem, *Zsch. f. Psychol.*, 3, 1892, 108; G. E. Müller, Zur Psychophysik der Gesichtsempfindungen, *Zsch. f. Psychol.*, 10, 1896, 321; F. Exner, Über die Grundempfindungen im Young-Helmholtzschen Farbensystem, *Sitzber. d. Akad. d. Wiss. zu Wien, math.-naturw. Kl.*, 111(2a), 1902, 857; E. Dreher, Methodische Untersuchung der Farbentonänderungen homogener Lichter bei zunehmend indirektem Sehen und veränderter Intensität, *Zsch. f. Sinnesphysiol.*, 46, 1911, 1; L. Richtera, Über die Änderung der Grundempfindungskurven mit der Intensität, *Sitzber. d. Akad. d. Wiss. zu Wien, math.-naturw. Kl.*, 122(2a), 1913, 1915; H. Goldmann, Über Dauerwirkungen farbiger Lichter auf das Auge, *Arch. f. d. ges. Physiol.*, 210, 1925, 70; B. Kern, Die bei Steigerung oder Verminderung der Lichtintensität eintretenden qualitativen Änderungen der Farben, in B. Kern and F. Schöne, *Sonderstellung gewisser Farbtöne und Heilbehandlung der Farbenschwäche, Abhandlungen aus dem Gebiete der Psychotherapie und medizinischen Psychol.*, herausg. von A. Moll, Heft 2, 1925; L. Janicki and E. Lau, Über die Abhängigkeit der Farbe von der Intensität, *Zsch. f. Sinnesphysiol.*, 57, 1926, 288; A. Brückner, Zur Frage der Eichung von Farbensystemen, *Zsch. f. Sinnesphysiol.*, 58, 1927, 322; F. Broer, Über das Purkinjesche Phänomen im Nachbild, *Zsch. f. Psychol.*, 113, 1929, 9; L. T. Troland, chap. 4 in *The Foundations of Experimental Psychology*, 1929; G. E. Müller, *Über die Farbenempfindungen*, Bd. 1, §25 (*Zsch. f. Psychol.*, Erg.-Bd. 17, 1930).

[6] A. König and C. Dieterici, Die Grundempfindungen im normalen und anormalen Farbensystemen und ihre Intensitätsverteilung im Spectrum, *Zsch. f. Psychol.*, 4, 1892, 241.

Exner[7] perceived that the Bezold-Brücke phenomenon suggests a method for precisely determining the intersection points of the three sensation curves. These curves, as plotted by König and Dieterici, have four intersection points: (a) that of the red and green curves at 578 mμ, (b) that of the red and blue curves at 505 mμ, (c) that of the green and blue curves at 490–500 mμ, and (d) that of the red and green curves at 470 mμ. The first point, since it represents equality between the 'red' and 'green' processes, must correspond to a hue which is complementary to the fundamental blue. The second corresponds to the hue of the fundamental green, the third to a hue complementary to the fundamental red, and the fourth to the hue of the fundamental blue.

Exner reasoned as follows. Consider, for example, the first intersection point, where the 'red' and 'green' processes are equal. A wave length on the long-wave side of this point will excite more 'red' than 'green.' If this wave-length is sufficiently reduced in intensity, the green component will sink below the threshold before the red component does; hence this light will become reddish as it is weakened. A wave-length on the short-wave side of the intersection point, on the other hand, excites more 'green' than 'red,' and the effect of lowering the intensity will be an increased greenishness. But the wave-length corresponding to the intersection point will be invariable in hue. This wave-length can be exactly identified by virtue of the fact that the hue changes on either side of it are in different directions. In the same fashion the three other intersection points may be located, and thus the three *Grundempfindungen* may be determined.

Experimentally, comparing a bright spectrum with a juxtaposed dim spectrum, Exner found three wave-lengths which did not vary in hue, and these were (a) 577, (b) 508, and (d) 475 mμ, in the yellow, the green and the blue respectively. The identification of the point *c*, in the blue green, was found to be too difficult, but its position could be calculated approximately when *b* and *d* were known.

These figures are in good agreement with König and Dieterici's intersection points. Exner regarded his results as furnishing a very accurate determination of the three fundamental sensations. He concluded that the curves of König and Dieterici required a slight revision, and should be replotted in terms of the fundamentals: (1) red—a purplish red complementary to 494 mμ, (2) green—508 mμ, and (3) blue—475 mμ.

In spite of this excellent agreement, Exner's interpretation of the Bezold-Brücke phenomenon in terms of the trichromatic theory is open to criticism.

In the first place, if the hue changes are caused by the fact that one component of the excitation process falls below the threshold, one would expect such changes to occur only at the very lowest intensities. Actually, however, the spectral hues change continually with intensity throughout a wide intensity range.[8]

We may seek to formulate a more satisfactory theory by adopting a suggestion made by Charles S. Peirce.[9] Peirce pointed out that if there are three independent chromatic processes, it is most likely that they will vary with the intensity of the light not in direct proportionality, but according to a law of diminishing returns. We need not follow Peirce in assuming that the law in question is that of Fechner,

[7] *Op. cit.*

[8] My own observations in this respect confirm those of Janicki and Lau (*op. cit.*).

[9] *Op. cit.* A somewhat similar though more arbitrary assumption was made by Helmholtz, *loc. cit.* (note 2).

but some law of this general type seems to be demanded, in view of our knowledge regarding the behavior of physiological excitations in general as functions of their stimuli.[10]

Now, let us see what follows from this assumption. Suppose, for example, that a certain long-wave spectral light excites the 'red' and the 'green' processes, and the former in greater degree than the latter. The hue which is experienced is, on Young-Helmholtz principles, dependent upon the ratio between the two processes. If the intensity of the light is increased, the 'green' excitation, being the lesser, will be increased relatively more—by a greater percentage of itself—than will the 'red.' This follows from our law of diminishing returns. Hence the ratio of 'red' to 'green' will be decreased by raising the intensity. The experienced hue will therefore become less reddish, that is, more yellowish. Conversely, if the intensity is decreased, the hue will become more reddish (less yellowish).[11]

It will readily be seen that the hue changes which this theory predicts are exactly the same as would follow from Exner's conception, but the theory has the advantage, as against Exner's, of explaining a continuous series of hue changes corresponding to a continuous variation of light intensity.

Unfortunately, we find that our theory meets with a contradiction when we consider the well-known experimental fact of the "additivity of brightnesses."

Careful experiments by Abney, Ives, Exner, and others have shown that the brightness of a mixture of lights is always equal to the sum of the brightnesses of its components, regardless of the hues of those components. Let us consider a special case. Suppose we have a red light and a green light which are matched in brightness. Now, if to each of these we add the same quantity of green light, the two mixtures will be equal in brightness. But on the law of diminishing returns, this should not be the case. Rather, the mixture of red and green should appear brighter than the mixture of two green lights. For in the former mixture the addition, as regards the retinal process, is primarily to a different excitation from that which originally existed. In the latter mixture, on the other hand, we are adding to the same retinal excitation, namely the 'green', and the increment stimulus will, according to the law of diminishing returns, produce less effect than it would if no 'green' excitation already existed.

Thus it appears to the writer that the Bezold-Brücke phenomenon, taken in conjunction with the fact of the additivity of brightnesses, presents a serious difficulty for the trichromatic theory.

We now come to a second difficulty, which appears to have been strangely overlooked by Exner. Namely, we should expect a spectral light, when reduced in intensity, to shift in hue in the direction of the dominant *Grundempfindung* (except when the wave-length corresponds to an intersection point of the sensa-

[10]Hecht, in his theory of visual response, assumes that the individual chromatic processes vary with intensity according to a law which coincides approximately with Fechner's within a middle range of intensities. Cf. S. Hecht, The relation between visual acuity and illumination, *J. Gen. Physiol.*, 11, 1928, 255.

[11]If a spectral light excites all three chromatic processes, the hue will of course be determined by the ratio between the two which are excited most strongly. The weakest excitation will contribute only to the white component (desaturation) of the color. On the theory outlined, a color should become progressively less saturated as the light intensity is raised, and more saturated as it is lowered. Cf. the writer's discussion of this point in the paper cited above.

tion curves). As a matter of fact, the hues of the longer wave-lengths shift towards red, and those of the middle wave-lengths shift towards green, under intensity diminution. But the hues of the short-wave end of the spectrum shift towards violet, and not towards blue. If a blue corresponding to 475 mμ (practically a pure blue with no tinge of violet) is the dominant *Grundempfindung* in this region, as Exner supposed, then the bluish violet hues ought, on the theory, to become more bluish, rather than more reddish, as the light is weakened.

This difficulty is not to be escaped by postulating violet, rather than blue, as the third fundamental sensation. For on that hypothesis the invariable hue ought not to appear in the pure blue at 475 mμ.

It would seem then that the Bezold-Brücke phenomenon cannot be satisfactorily explained in terms of the trichromatic theory in the way proposed by Exner.

If we attempt a similar explanation, using Hering's theory rather than Helmholtz's as our starting point, we are still faced by the same difficulty in regard to the behavior of the lights of short wave-length. For, on the former theory as well as the latter, these lights produce a mixture of 'blue' and 'red' excitation, with the 'blue' in excess of the 'red,' and we should expect a hue shift towards the blue when the intensity is decreased.

Hering[12] maintained that the three invariable colors in the spectrum are identical with the 'psychological primaries' or *Urfarben*, the pure yellow, the pure green and the pure blue, which, together with the pure red (lying outside the spectrum) he regarded as the elementary color sensations. Careful determinations of the psychological primaries by Westphal[13] in fact locate them at the following wave-lengths: for yellow, 574.5, for green, 505.5, and for blue, 478.5 mμ. These results are quite close to Exner's values for the invariable hues, viz. 577, 508 and 475 mμ.

The experimental work to be reported in the present paper represents a study of the Bezold-Brücke phenomenon in a number of different aspects.

In the experiments to be described first, I have made certain quantitative measurements of the phenomenon. The results obtained give one an idea as to the magnitude of the error which is committed when one assumes that spectral hue is uniquely determined by wave-length, and disregards the influence of intensity. These measurements involved incidentally a redetermination of the invariable hues in the spectrum.[14]

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[12] *Op. cit.*

[13] H. Westphal, Unmittelbare Bestimmung der Urfarben, *Zsch. f. Sinnesphysiol.*, 44, 1909, 182. Cf. L. T. Troland, Report of the Colorimetry Committee of the Optical Society of America, *J. Opt. Soc. Amer.*, 6, 1922, 531.

[14] In these experiments, as well as in those to be described later, the writer was the only observer. This limitation naturally imposes a certain caution upon us in generalizing from the results.

The 'Apparent Shift in Wave-Length' and the Invariable Hues

In this work, judgments were made upon homogeneous fields of monochromatic light, rather than upon complete spectra as in the experiments of Exner. One half of a circular three-degree field[15] was illuminated by monochromatic light of fixed intensity, and the other half by monochromatic light furnished by a different source and having a fixed lower intensity. If the light on both sides had the same wave-length, then, in accordance with the Bezold-Brücke phenomenon, the hues on the two sides would in general be dissimilar. The hues could, however, be made to match by changing the wave-length of one source of light. The alteration in wave-length required to produce a hue match could then be used as a quantitative expression of the Bezold-Brücke shift corresponding to the given intensity difference.

The hue matches which one thus obtains are never perfect. For example, a yellow at low intensity has a brownish tinge which is not found in a brighter yellow. Also the difference in brilliance and saturation between the two sides of the field makes the comparison exceedingly difficult, and much practice is required in order to make the judgments. The use of an intensity ratio greater than 20:1 was found to be impracticable.

The details of the procedure were as follows. The spectroscope illuminating one half of the field was adjusted to the desired wave-length, say 600 mμ (orange), and the voltage of its lamp was so adjusted as to yield an intensity of, say, 1000 photons. The spectroscope illuminating the other half of the field was set at a wave-length well into the red end of the spectrum. The voltage of its lamp was so adjusted that the stimulus intensity on the corresponding side of the field had a value of, say, 100 photons. The wave-length setting of this latter spectroscope was then gradually decreased, so that the hue difference between the two sides of the field became progressively less. Any alteration of wave-length would, of course, involve a simultaneous alteration of intensity. Hence, after each small variation in the wave-length, the voltage of the lamp was adjusted so as to restore the initial intensity level of 100 photons. A calibration curve, representing the voltage required to produce 100 photons as a function of wave-length, provided the most convenient means of making this adjustment. This process was continued until that wave-length of the 100-photon stimulus was found which presented a maximum of similarity to the 1000-photon stimulus of 600 mμ. Then the process was repeated, this time beginning with a wave-length of the 100-photon stimulus considerably less than 600 mμ, and gradually increasing the wave-length until a match was obtained.

[15]For a description of the apparatus, see my previous paper (*op. cit.*).

The best match would be found at a wave-length shorter than 600 mμ, because a reduction of intensity makes the orange hues become redder. The average setting was in fact about 594 mμ. The Bezold-Brücke change, expressed in terms of wave length, was −6 mμ.

In this way a series of matches was obtained between wave-lengths at 1000 photons and wave-lengths at 100 photons. The results could be represented by a curve showing wave-length difference as a function of the wave-length of the brighter stimulus, due account being taken of the sign of the difference.

In similar fashion, hue comparisons were made between spectral stimuli of the following intensities: 2000 and 400 photons, 2000 and 100 photons, 400 and 100 photons, 400 and 20 photons, 100 and 10 photons. The curves were essentially alike in all these cases, although the absolute value of the Bezold-Brücke shift was naturally greater with large brilliance differences than with small. So far as one can judge from these limited data, the amount of the shift appears to depend more closely upon the ratio between the two intensities than upon their arithmetical difference. In other words, it depends upon the 'psychological' rather than the 'physical' intensity difference.

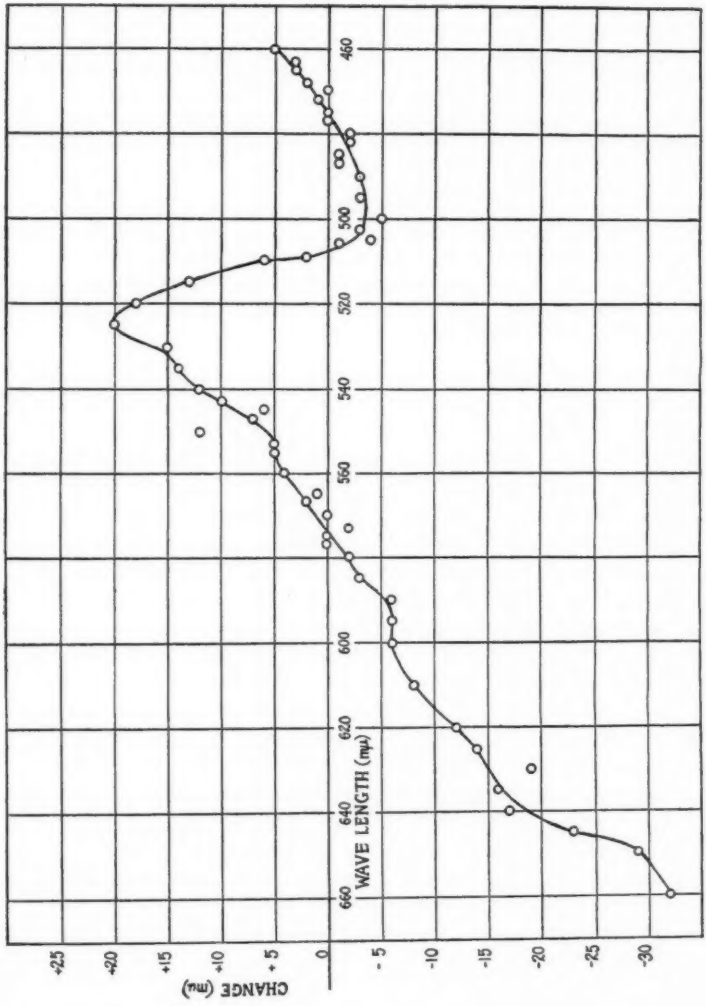
In Fig. 1[16] the results of the comparisons between 1000- and 100-photon stimuli are graphically represented. It will be seen, to begin with, that the curve has negative ordinates in the long-wave end of the spectrum (yellowish red region). A spectral light in this region becomes more reddish as the intensity is decreased, and one has to diminish the wave-length in order to restore the original hue. As one passes to shorter wave-lengths, and more yellowish hues, the change with intensity becomes less and less, until finally, at a wave-length of about 574 mμ, the hue is invariable with respect to intensity, *i.e.*, the ordinate of the curve is zero.

Continuing to shorter wave-lengths (in the greenish yellow), one finds that decreasing the intensity causes the hue to become more greenish. A given stimulus of high intensity must be matched by a low-intensity stimulus of longer wave-length, *i.e.* a wave-length nearer the pure yellow. The ordinates of the curve are now positive.

This positive wave-length displacement reaches a maximum in the yellow-green, at about 525 mμ, and then declines. At 507 mμ the curve again crosses the axis; this is a second invariable point.

Beyond 507 mμ, in the blue green region, the hue once more shifts towards green when the intensity is lowered. When a low-intensity and a high intensity stimulus are matched in hue, the former must have the shorter wave-length. The curve therefore has negative

[16]This graph has already been published in schematic form by Troland, *op. cit.* (note 5), 199, Fig. 9, but the diagram as there presented contains an error, the ordinates being only one-half their proper size.



WAVE-LENGTH OF 1000-PHOTON STIMULUS

FIG. 1. GRAPH OF THE BEZOLD-BRÜCKE PHENOMENON

(The ordinates represent the change in wave-length required in order to produce the same hue at 100 photons.)

ordinates in this region. The displacement here is very slight, and it soon reaches its greatest amount and declines to zero. A third invariable point occurs at 476 mμ.

Finally, in the short-wave end of the spectrum, the hue shifts from blue towards violet with diminution of intensity. The curve again has positive ordinates.

There are thus three wave-lengths which yield invariable hues, one in the yellow, one in the green, and one in the blue. On two of the graphs in addition to the one here presented (Fig. 1), the positions of the invariable points could be determined with a fair degree

TABLE I

THE INVARIABLE POINTS IN THE SPECTRUM

Intensities compared	Invariable points		
	(1)	(2)	(3)
(1) 2000 and 100 photons	570 mμ	507 mμ	475 mμ
(2) 1000 " 100 "	574	507	476
(3) 400 " 20 "	570	504	471
Average	571	506	474

of certainty. They are shown in Table I. Comparing these values with Exner's, viz., 577, 508 and 475 mμ, we find a fair measure of agreement.

In regard to the magnitude of the Bezold-Brücke effect, it will be seen from the graph that the wave-length difference reaches very considerable values in some parts of the spectrum, being over 30 mμ for red light of wave-length 660, and about 20 mμ for yellow-green light of wave-length 525. It must of course be realized that a representation of the phenomenon in the physical terms which we have employed does not convey a true picture of its magnitude in terms of shift in hue, since sensitivity to wave-length difference varies in different parts of the spectrum. Moreover, by our method of plotting, the spectrum is divided into four parts as regards the Bezold-Brücke effect—two of negative and two of positive ordinates. Psychologically, it is divided into three regions—one in which the hue shift accompanying intensity reduction is towards the red, a second in which it is towards the green, and a third in which it is towards the violet.

THE BEZOLD-BRÜCKE EFFECT FOR EXTRA-SPECTRAL HUES

Between the yellowish reds at the long-wave end of the spectrum and the reddish blues at the short-wave end there is a species of inter-

mediate tones, not found in the spectrum, which can be produced by mixing spectral red and blue (or violet) in different proportions. These hues form a series of gradual transitions from violet through purple, carmine and pure red to spectral red, and complete the 'color circle' which the spectrum leaves incomplete.

In the case of the colors at both end-regions of the spectrum, we find that a reduction of intensity causes an increased reddishness. In each case we note that the Bezold-Brücke shift increases in amount as the limit of the spectrum is approached. Now, it is of interest to know how the extra-spectral intermediate hues will behave as regards the Bezold-Brücke effect. By observing the hue changes of these colors we can bring the two ends of our curve (Fig. 1) into relation with each other; and we shall be able to state the law of the Bezold-Brücke effect in its most general form.

For the purpose of studying the extra-spectral colors a mixture of radiations from two spectroscopes was employed, the intensities of the two lights being separately controlled by means of rheostats in series with the lamps illuminating the respective slits. The field size as before was 3°. One half of the stimulus area was reduced in intensity by a series of thin glass plates (microscope cover glasses) inserted in the path of the rays. Two stimuli of the same spectral composition but different intensities could thus be compared.

The proportion between long-wave and short-wave radiation was varied by small degrees, and the hues of the brighter and fainter halves of the field were compared after each such variation.

Beginning with a spectral yellowish red and adding blue to it in gradually increasing amount, the Bezold-Brücke shift (towards red) was soon found to decrease, and eventually to become zero.

Thus, in addition to the three spectral invariables, there is a fourth invariable hue within the series of extra-spectral colors.

As the proportion of blue was still further increased, the hue-displacements were once more towards the red, and continued in that direction, regardless of the amount of blue which was added. In other words, the Bezold-Brücke changes for the extra-spectral reddish blues are directly continuous with those of the spectral violets.

Considering the total hue series or color circle, embracing both the spectral and the extra-spectral hues, we conclude that it can be divided into two regions as regards the Bezold-Brücke effect. In the first region, which includes the yellowish and bluish greens, the hue

shifts towards green with reduced intensity. In the second region, which includes all other colors, the effect of diminishing the light is to shift the hue towards the red.

On Exner's theory, there should be three regions—one in which the hue shift is towards red, one in which it is towards green, and one in which it is towards blue. This third region should include the spectral violets, and a certain proportion of the extra-spectral purples. As spectral violet is made increasingly redder by the addition of long-wave light, the Bezold-Brücke shift should be directed towards blue until such a mixture is reached that the 'red' and 'blue' excitations are equal; this mixture should be invariable in hue with respect to intensity. Beyond this point the colors would be expected to turn redder when reduced in intensity. Finally, when the 'blue' excitation is so low, relatively, as to be just equal to the 'green' component of the spectral red, another invariable point should occur. This point would represent the hue of the red *Grundempfindung*. For the yellowish red hues beyond this second invariable point the Bezold-Brücke shift should once more be towards the red.

The first of these predicted extra-spectral invariables is, of course, not found in actual experiment, since there is no hue-shift in the direction of blue. As to the invariable which was found, let us attempt to determine, as well as we can, whether it is identical with the red *Grundempfindung*.

We can estimate the degree of agreement in the following way. On the Young-Helmholtz theory, the red *Grundempfindung* corresponds to a condition of equality between the 'blue' and 'green' excitations. Now, for the particular spectral mixture which was experimentally found to be invariable in hue, we can calculate the 'blue' and 'green' excitation values, and determine whether their ratio approximates the theoretical value of 1. This calculation is made possible by the work of Exner,[17] who has furnished data for the translation of stimulus intensities of spectral lights into excitation values.

In one determination of the invariable hue I mixed light of 639 mμ with light of 427 mμ. The former was held constant, while the amount of the latter stimulus was varied until the brighter section of

[17]F. Exner, Zur Kenntnis der Grundempfindungen im Helmholtzschen Farbensystem, *Sitzber. d. Akad. d. Wiss. zu Wien, math.-naturw. Kl.*, 129 (2a), 1920, 27.

the field matched the darker in hue. The intensities giving an invariable mixture, as measured by the flicker method, were

639 mμ.....420.0 photons
427 mμ (mean of 5 determinations).....1.0 photon

The 'blue' and 'green' excitations, according to Exner, have the following relative values for these two wave-lengths:

639 mμ..... { Blue = 0 (practically); Green = 8.2 }
427 mμ..... { Blue = 105; Green = 0 (practically) }

Exner has found that the "chromatic visibility coefficients" of the 'green' and 'blue' excitations are in the ratio 31.5:1, *i.e.* it takes 31.5 times as much green, in terms of intensity, to produce the same effect, in terms of excitation, as a given amount of blue. Therefore the excitation ratio in the mixture is:

$$\frac{\text{Blue}}{\text{Green}} = \frac{105 \times 1 \times 31.5}{8.2 \times 420} = 0.96$$

I have repeated the experiment with a different pair of wave-lengths, 616 and 464 mμ, obtaining the following intensities for the invariable mixture:

616 mμ.....560 photons
464 mμ (mean of 8 determinations)..... 3.1 photons

For these wave-lengths the 'blue' and 'green' components are respectively:

616 mμ..... { Blue = 0 (practically); Green = 25.0 }
464 mμ..... { Blue = 129; Green = 0 (practically) }

The excitation ratio is then:

$$\frac{\text{Blue}}{\text{Green}} = \frac{129 \times 3.1 \times 31.5}{25 \times 560} = 0.90$$

Thus in one calculation we obtain 0.96, and in the other 0.90, as compared with the theoretical ratio of 1.00. The observed invariable mixture is thus in approximate agreement with one of the invariable mixtures predictable from the Young-Helmholtz theory, namely a mixture which produces equal amounts of 'blue' and 'green' excitation, and which evokes the hue of the red fundamental sensation.

It must be realized, of course, that there is considerable uncertainty involved in such a calculation, especially in view of possible individual differences in the excitation curves and in the chromatic visibility coefficients.

Although the positions of the four invariable points, three spectral and one extra-spectral, are consonant with the trichromatic theory, nevertheless, as I have pointed out, the Bezold-Brücke phenomenon in its entirety cannot be readily harmonized with that theory.

The Relation of the Invariable Hues to the Psychological Primaries

We have mentioned Hering's idea that the invariable hues are identical with the psychological primaries or *Urfarben*. In point of fact, both Exner's and my own determinations of the spectral invariables agree approximately with Westphal's data upon the *Urfarben*. A correlation between the results of different workers in color vision experiments is, however, always subject to more or less uncertainty. It seemed worth while, therefore, to make determinations of the spectral *Urfarben* for my own eye, and then to compare them with my own data upon the invariable colors.

I have made determinations of these colors at several different levels of intensity. Strictly speaking, I determined the 'critical hues' in the spectrum, *i.e.* the places at which the change of hue with wave-length suddenly alters its direction. Thus, beginning with the extreme red, one observes that the hues become more yellowish with decreasing wave-length, until a certain critical wave length is reached beyond which the change is in the direction of the green. Besides this 'critical yellow,' the spectrum contains a 'critical green' and a 'critical blue.'

In these observations the intensity level was held constant by frequently checking the spectral light against a photometric standard of some predetermined intensity. But, as a matter of fact, the spectral positions of the *Urfarben* were not found to vary significantly with intensity within the range employed.

One finds a different wave-length for the *Urfarbe* according as one approaches it from the long- or short-wave side. Part of this discrepancy is probably due to chromatic fatigue, and part of it to variations in one's mental standard of judgment. In order to balance out this source of variation, I have made determinations from either side alternately, and averaged the results.

In each case observations were made, with the results given in Table II.

The values obtained are very near Westphal's, viz., 574.5, 505.5 and 478.4 mμ. Moreover, the average positions of the invariable hues for my own eye were 571, 506, and 474 mμ. This there is fair agreement between the spectral *Urfarben* and the invariables as determined by the same observer, the coincidence being most dubious in the case of the yellow.

TABLE II

THE SPECTRAL *URFARBEN*

(Values in mμ)

Intensity	Wave-Lengths of *Urfarben*					
	Yellow		Green		Blue	
	Mean	A.D.	Mean	A.D.	Mean	A.D.
10 photons	576	2.9	503	2.0	476	1.4
100 photons	575	3.5	504	2.1	478	2.5
1000 photons	576	3.5	504	3.6	474	3.3
Average	576		504		476	

However, the fourth invariable hue, obtained by mixing spectral red and violet, was observed to have a definite though slight purplish tinge, and consequently is not identical with the *Urrot*. On this account we cannot assume a complete coincidence between the two classes of hues.

THE NUMBER OF HUES VISIBLE AT THE THRESHOLD OF COLOR VISION

The term *Bezold-Brücke phenomenon* is frequently understood in the sense that at the lowest intensities compatible with color vision there are only three hues visible, red, green, and violet. Von Bezold's observations indicated this to be the case; it is to be noted, however, that he employed a complete spectrum in his experiments, rather than isolated patches of monochromatic light such as I have used in the experiments described here. Now, with a band of light of continuously changing hue, one's discrimination of hue differences is much inferior to that obtained when isolated homogeneous patches of color are compared.

In order to ascertain whether, under the latter condition, the number of distinguishable hues actually reduces to three, I have employed the following method. Two semicircular patches of mono-

chromatic light were presented in juxtaposition, as in the first series of experiments. The wave-lengths in the two halves of the field were so adjusted as to differ by 10–25 mμ. The intensity on each side was then carefully reduced until a level was reached such that any further reduction would cause the color of the light to become imperceptible. A judgment was then made as to the hue of each of these threshold colors.

TABLE III
COMPARISONS OF LIMINAL HUES

L	S	Hue of L	Hue of S
(Values in mμ)			
625	600	Red	Yellow
600	590	Red	Yellow
590	580	Red	Yellow
580	570	Orange yellow	Greenish yellow
570	560	Orange yellow	Yellow
560	550	Yellow	Green
550	540	Yellow	Green
540	520	Yellow	Green
520	510	Greenish yellow	Green
510	500	Yellow	Green
500	490	Yellowish green	Green
490	480	Green	Violet
480	470	Green	Violet
470	450	Blue	Violet
450	430	Blue	Violet

In Table III the results for the various stimulus-pairs are listed. In the first column, marked "L," is given the member of the pair having the longer wave-length, and in the second column, marked "S," the member having the shorter wave-length. The third and fourth columns contain the judgments as to the respective hues.

On the basis of these results we can say that the number of hues in the spectrum is not reduced to three, even at the very lowest intensities at which color can be seen at all. As a matter of fact, in most parts of the spectrum lights differing in wave length by 10 mμ (how much less I have not attempted to determine) can still be discriminated. The unreliable method used by von Bezold is accountable for his conclusion.

THE CHANGE OF HUE BY ADDITION OF WHITE

The hue of a color may be altered not only by changing its brilliance, but also by adding white light to it, which, *a priori*, might

be expected to affect only the saturation. This curious effect is especially striking in the case of the blues and violets; these hues become very distinctly redder upon dilution with white. The phenomenon was first observed by Aubert,[18] and has received frequent casual mention in the literature. Helmholtz supposed the hue changes to be similar to those which occur in the Bezold-Brücke phenomenon, the tendency in each case being towards an increased predominance of the three *Grundempfindungen*, and a reduction of the spectrum to three monochromatic patches.

Abney is apparently the only experimenter who has made systematic observations with spectral colours. He describes the phenomenon in these words: "In the red the colour becomes pinker as more of the white light is added, the scarlet becomes orange, the orange yellow, and the yellow green. The yellow-green does not suffer a change, but as the green is approached it becomes yellower in hue, and as the white light passes over the green, this same tendency to yellowness appears. In the blue there is not much alteration, but as the violet is approached a very small quantity of white will make it appear nearly salmon-coloured."[19] He reports that there is a certain hue in the yellow, at 577 mμ, which is not changed by the addition of white. "In the blue-green as far as λ 508 the colour shifts in hue towards the yellow. In the true blue-green, about λ 489, the addition seemed to make no difference in hue, simply making it appear rather paler. At λ 489 the mixture of colour and white made the blue become redder."[20]

In my own observations, adding white to a homogeneous three-degree field, I obtained the results of Table IV.

These observations are in broad agreement with Abney's, except in the case of the red, which I found to acquire a slightly purplish tinge when mixed with white,[21] and the yellow region, in which I could not verify the shift towards green which Abney reports. The hue changes of the colors at either end of the spectrum are more easily observed than those of the middle colors; this is at least partly due to the fact that the former retain their saturation in greater degree when white is added to them.

[18]H. Aubert, *Physiologie der Netzhaut*, 1865, 136. See also: E. Brücke, Über Ergänzungsfarben und Contrastfarben, *Sitzber. d. Akad. d. Wiss. zu Wien, math.-naturw. Kl.*, 51(2), 1865, 461; O. N. Rood, *loc. cit.*; *idem*, On the effects produced by mixing white with colored light, *Amer. J. Sci.*, Ser. 3, 20, 1880, 81; H. Kuhnt, Über farbige Lichtinduction, *Arch. f. Ophth.*, 27(3), 1881, 5; H. von Helmholtz, *op. cit.* (note 5); F. Brentano, *Untersuchungen zur Sinnespsychologie*, 1907, 135; G. Révész, Über die vom Weiss ausgehende Schwächung der Wirksamkeit farbiger Lichtreize, *Zsch. f. Sinnesphysiol.*, 41, 1907, 102; W. de W. Abney, *Researches in Colour Vision and the Trichromatic Theory*, 1913, chap. 17; E. Schrödinger, Grundlinien einer Farbenmetrik im Tagessehen, *Ann. d. Phys.*, 63, 1920, 481; B. Kern, *op. cit.*

[19]*Loc. cit.*, 255. [20]*Ibid.*, 257.

[21]Rood, Helmholtz, Schrödinger, and Kern (*opp. citt.*) report the same.

The hue changes found here are not strictly parallel to those which occur in the Bezold-Brücke phenomenon. The yellow-green hues become yellower upon the addition of white, although they become greener upon diminution of intensity. The pure blue or *Urblau* turns reddish when white is added to it, and yet is unaffected by variations of intensity. Spectral red, diluted with white, looks distinctly bluish as compared with the undiluted color at weak illuminations. Generally

TABLE IV

THE EFFECTS OF ADDING WHITE LIGHT TO SPECTRAL COLORS

Wave-Length (mμ)	Hue becomes
650 (red)	purplish
620 (orange-red)	purplish
610 (orange)	pinkish
600 (orange)	pinkish
590 (orange)	pinkish
580 (yellow)	unchanged
570 (yellow-green)	yellower
560 (yellow-green)	yellower
550 (yellow-green)	yellower
540 (yellow-green)	yellower
520 (green)	yellower
500 (green)	unchanged
490 (blue-green)	unchanged
480 (blue)	pinkish
470 (blue)	pinkish
460 (violet)	pinkish

speaking, the admixture of white appears to produce an effect on the hue which is equivalent to that of mixing bluish red light with the spectral stimulus.[22] It is doubtful, then, whether there is any intimate linkage between the Bezold-Brücke effect and the phenomenon of hue change due to the addition of white.

On the basis of the Young-Helmholtz theory, and the supposition that the individual chromatic excitations obey a law of diminishing returns, it will readily be seen that a hue shift would logically be expected to follow the addition of white, but that this expected shift would be similar to that accompanying a rise of intensity, *i.e.*, towards yellow or blue. The actual shift, as we have seen, is quite different.

[22]Cf. Rood, *opp. citt.*

The attempt made by Abney[23] to explain the phenomenon in terms of the trichromatic theory appears to the writer to be erroneous. Abney supposes the effect to be equivalent to a strengthening of the red and green components relative to the blue, and ascribes it to the fact that white light contains only a minute luminosity of the blue component as compared with the red and green. It must be noted, however, that this small quantity of blue is not negligible in respect of chromatic power, and therefore cannot be disregarded in estimating the hue of the mixture of spectral color and white. Moreover, on the empirical side, the effect of adding white seems to correspond to an enhancement of the red and blue components rather than of the red and green.

In terms of the Hering theory, which assumes a white process independent of the chromatic processes, it is difficult to see how the phenomenon can be made intelligible.

It would appear that both the white-mixture phenomenon and the Bezold-Brücke phenomenon, involving an inconstancy of hue at constant wave-length, are of great significance for the theory of color vision, and should be carefully considered in connection with its fundamental postulates.

Summary

(1) Quantitative observations on the Bezold-Brücke phenomenon, or change of hue with intensity, are reported. The method consisted in matching the hue of a weak stimulus with that of an intense stimulus by introducing an appropriate wave-length difference. The results are expressed by a curve representing wave-length difference as a function of wave-length.

(2) Three wave-lengths in the spectrum were found to be invariable in hue with respect to intensity. These were located at about 571, 506, and 474 mμ, in the yellow, the green and the blue respectively, in approximate agreement with the determinations made by Exner.

(3) The Bezold-Brücke shift was also observed in the case of the extra-spectral colors produced by mixing spectral red and violet. For these colors the shift takes the form of an increasing reddishness. But a certain mixture can be found which does not change in hue with intensity. Thus there are altogether four invariable hues.

[23] *Loc. cit.*

(4) Calculation of the trichromatic excitations for this fourth invariable hue brings out an approximate agreement with the red fundamental sensation of the Young-Helmholtz theory.

(5) The spectral *Urfarben* were found to occur at about 576, 504 and 476 mμ, not far from the positions of the invariable hues. The extra-spectral invariable was, however, a purplish red and not an *Urfarbe*. Thus the writer cannot verify Hering's notion that the *Urfarben* and the invariable hues are identical.

(6) Von Bezold's statement that there are only three hues (red, green and violet) in the spectrum at the lowest intensities is not corroborated under better experimental conditions.

(7) The hue changes undergone by the spectral colors when diluted with white light were observed, and found to display differences from those occurring under reduction of intensity.

(8) Neither the Bezold-Brücke phenomenon nor the white-mixture phenomenon can be readily reconciled with the Young-Helmholtz or the Hering theory of color vision.

ON ABSOLUTE AND RELATIVE JUDGMENTS IN LIFTED WEIGHT EXPERIMENTS

By Samuel W. Fernberger, University of Pennsylvania

An adequate historical sketch of the problem of the absolute judgment from the time of Fechner until 1928 will be found in the paper by Wever and Zener.[1] From their own experimental work, reported in the same paper, they indicate that absolute judgments are "possible through the establishment for the *O*, by the presentation of a series of stimuli, of a certain degree of knowledge of that series." They find that the establishment of this series is rapid. This work was amplified by Gahagan[2] with an interesting experimental arrangement. This author concludes that "the limens (absolute) of the comparison positions, irrespective of the temporal distance from the standard, are similar to the normal limens (relative)."

Gahagan specifically notes that all of his *O*s, except one, had served in other lifted-weight experiments. Wever and Zener do not state, in their article, whether or not their *O*s were also trained in this respect. A letter from Wever states, however, that "aside from observers *W* and *Z*, who had done a very slight amount of weight lifting in some preliminary experiments, the observers in our absolute judgment experiments were unpracticed." It is obvious that an *O* who has already had training with a specific material will have already had established for him the 'absolute series' to a certain extent at least and will therefore know the particular universe in which he is judging. It has seemed worth while to determine what will happen with an absolute series and the judgments within that series if the *O* who approaches it is completely naïve not only to judgments in this field but to psychophysical procedures in general.

Furthermore, the author believes that neither of the experiments of Wever and Zener or of Gahagan completely and entirely investigated the absolute judgment in isolation. One finds that the first authors interpolated comparison series with the absolute series.

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[1] E. G. Wever and K. E. Zener, The method of absolute judgment in psychophysics, *Psychol. Rev.*, 35, 1928, 466-493.

[2] L. Gahagan, On the absolute judgment of lifted weights, *J. Exper. Psychol.*, 12, 1929, 490-501.

The method of Gahagan was even less adequate for an investigation of the absolute series in its true and isolated form. The method employed by Gahagan was to present a single standard stimulus followed successively by seven comparison stimuli. The belief was that as the limens were calculated for the different temporal positions from the standard stimulus the judgments more nearly approached the absolute form.

It seems to the author that the absolute judgment can only be investigated in its pure form, if it is divorced from any comparison judgments whatsoever and if the *Os* employed have never had any practice with comparison judgments, at least with the type of material presented. In order to conform to this belief, the following experiments were performed.

Observers. We were able to use as *Os* eight graduate students at the University of Pennsylvania who registered for a graduate training course in psychophysics. These observers were (*1*) W. W. M. Babb, (*2*) Sarah E. Crowell, (*3*) Eleanor W. Culin, (*4*) Elisabeth Davies, (*5*) Molly Goldman, (*6*) Paul K. Stolz, (*7*) S. S. Tomkins, and (*8*) Sarah Wood. Hereafter they will be known by number. All of these *Os* were entirely naïve at the beginning of the experiment. In the course in psychophysics, great care was taken to practice them on purely hypothetical and very varied materials for discussion and calculation. None of the *Os* had any previous practice in the experimental procedures employed and judgments were recorded from the very first lifting. They worked entirely without knowledge of the problem or the experimental procedure.

Apparatus. The apparatus consisted of a series of weights presented on a turning table-top. The rate of lifting was controlled by the beats of a metronome set at 92 per min. The standard weights (when employed) were always 100 grm. and the comparison weights were 84, 88, 92, 96, 100, 104 and 108 grm. A double series of comparison weights was used in carefully arranged irregular order to eliminate the effects of interserial expectation.[3]

Procedure. In the first part of the experiment, the comparison weights alone were presented to the *Os*. They were given preliminary practice of not more than 3 min. in the lifting of the 96 grm. weight (the middle one in the series) so that the hand movements would become reasonably automatic. They were then given the following instructions.

You will be presented with a series of weights which vary only slightly one from another. Some are light, some are heavy and some are intermediate. Immediately after the lifting of a weight, you will report whether it was light, intermediate, or heavy.

It will be observed that in these instructions we have used the descriptive term "intermediate" as more meaningful in this situation than the usual term

[3]For a more detailed description of the materials and manner or lifting, cf. S. W. Fernberger, A new form of stimuli for lifted-weight experiments, this JOURNAL, 31, 1920, 147-151; and On the relation of the methods of just perceptible differences and constant stimuli, *Psychol. Monog.*, 13, 1913, (no. 61), 6-11.

"equal." The word 'equal' always has a relative connotation which we especially wished to avoid. It will also be observed that there was no effort, in the instructions, to define the terms 'heavy,' 'intermediate,' or 'light.' There seemed to be no difficulty in the use of these terms on the part of any of the *O*s. Five series of 50 judgments on every weight were obtained from every one of the 8 *O*s. These will be known as Series I or the Absolute Series.

After the Absolute Series were completed, a Relative Series of the usual sort was obtained with the three-category plan of judgments. The standard weight of 100 grm. was presented first followed by one comparison weight and relative judgments were given in terms of the categories of 'lighter,' 'equal,' or 'heavier.' These categories were defined as follows in the instructions.

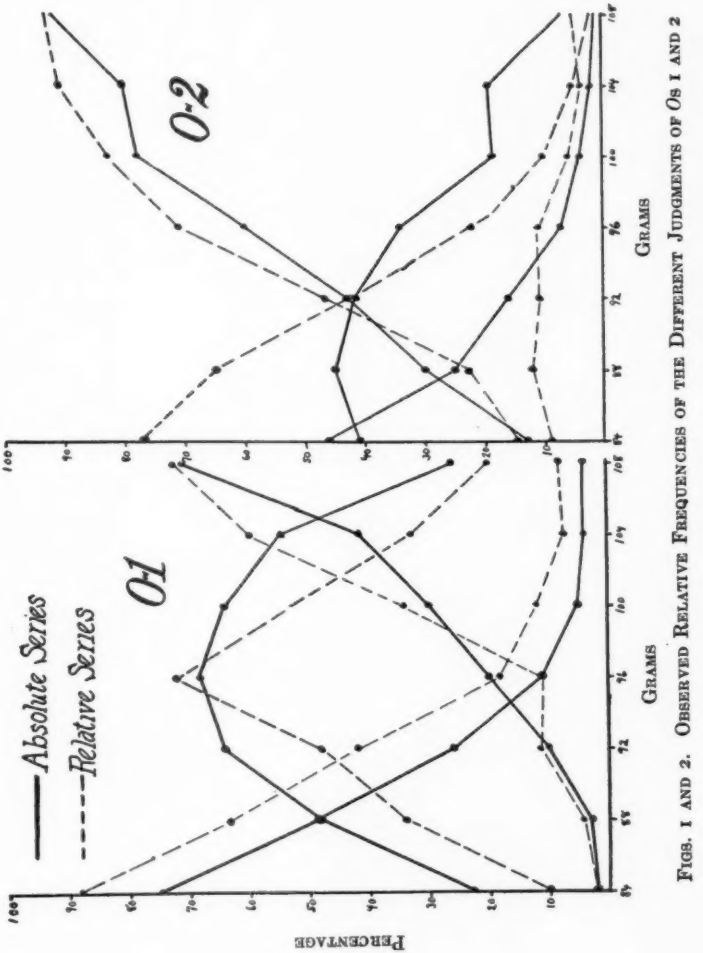
'Lighter' indicates that the second weight of the pair is lighter than the first weight of that pair; 'heavier' indicates that the second weight is heavier than the first weight of that pair; and 'equal' indicates that the two weights of any pair are subjectively equal, that you are unable to indicate a difference, or that you do not know.

Four series of 50 comparison judgments on every pair of weights were obtained from 7 of the *O*s and three similar series were obtained from *O 2* who at this time severed her connection with the University as a regular student. These will be known as Series II or the Relative Series. All of the series of judgments were treated statistically in accordance with Urban's method of calculation for the method of constant stimuli.

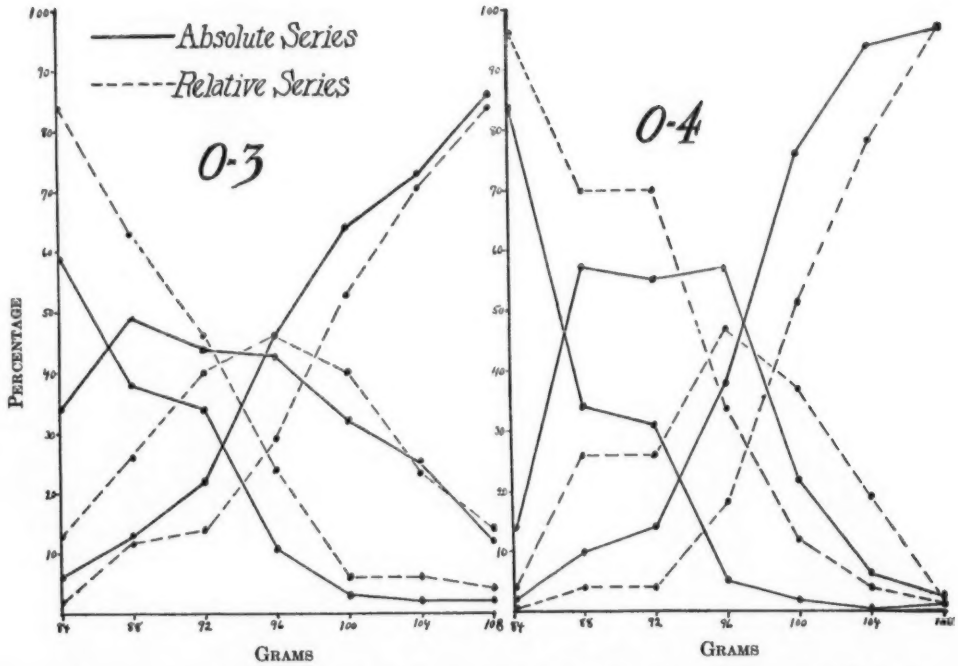
Results

Series I (*Absolute Series*). The results of the observed relative frequencies of the Absolute Series will be found for the total 250 judgments for each weight in Fig. 1–8. The curves for the psychometric functions of the Relative Series will also be found in the same figures and these will be discussed later. The curves of the broken lines are for the Relative Series; those of the solid lines for the Absolute Series which concern us for the moment. All the curves have a similar form. The stimuli are arranged along the abscissae and the observed relative frequencies are erected as ordinates. It will be observed that all of these curves of the Absolute Series more or less approximate the usual form of the psychometric functions. The curves for the 'heavy' and 'light' judgments are of the usual ogive form while that for the 'intermediate' judgments is of the bi-laterally symmetrical bell shaped form.

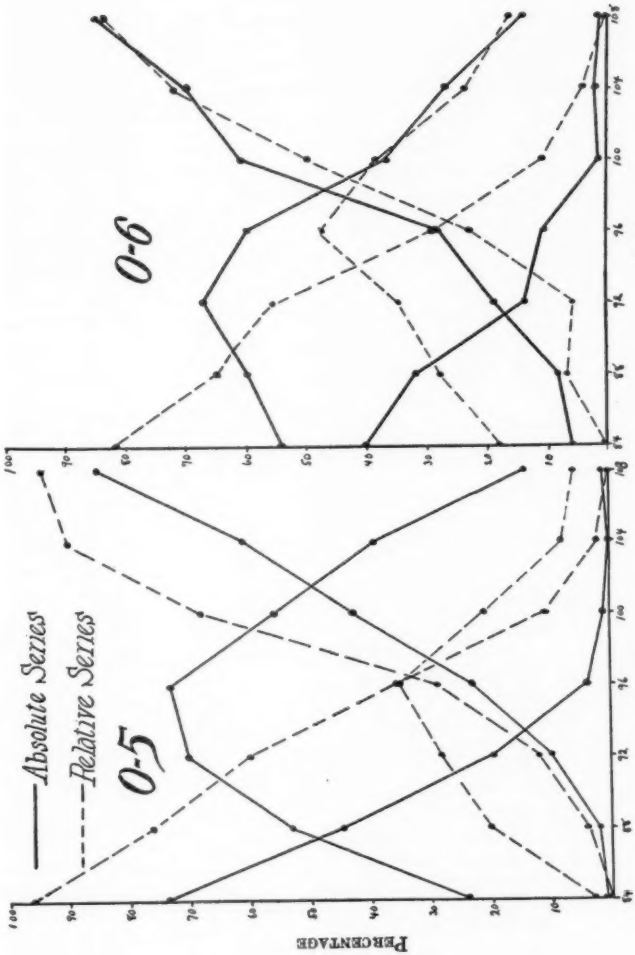
On the other hand the curves vary considerably in regularity. Those for *O 1* (Fig. 1) are very regular. Those for *O 2* (Fig. 2) are relatively regular (with one slight inversion—100 and 104 grm. weights of the 'intermediate' curve), and are all shifted toward the lower values of stimulus to the left of the abscissa. Those for *O 3* (Fig. 3) are somewhat less regular but present no inversions. The curve for the 'heavy' judgments of *O 4* (Fig. 4) is very regular, but the curves for her other two judgments are not—a slight inversion is found at the 92 grm. weight of



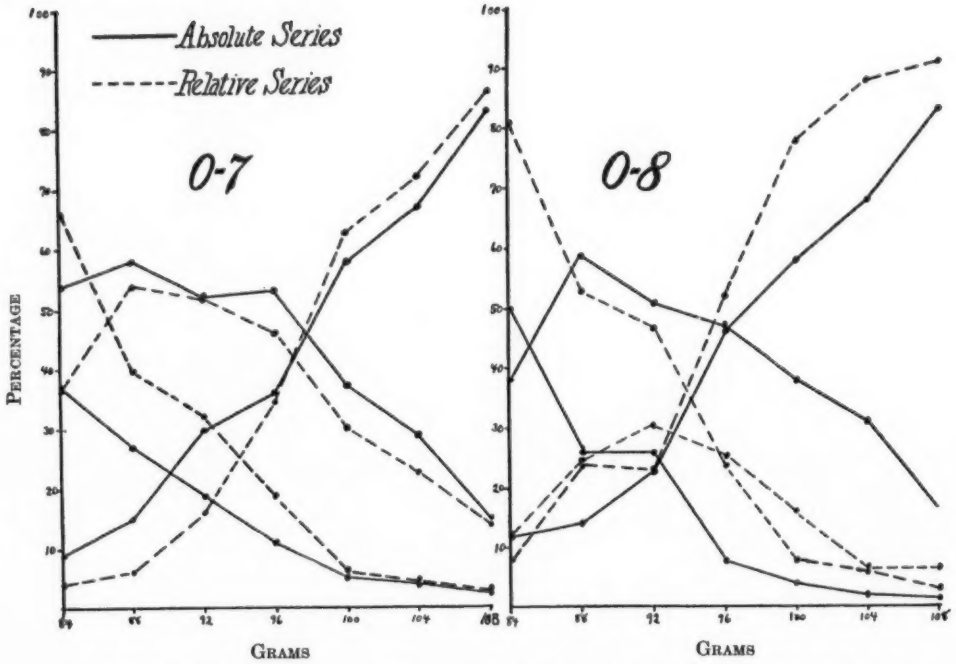
FIGS. 1 AND 2. OBSERVED RELATIVE FREQUENCIES OF THE DIFFERENT JUDGMENTS OF OS 1 AND 2



FIGS. 3 AND 4. OBSERVED RELATIVE FREQUENCIES OF THE DIFFERENT JUDGMENTS OF *O*S 3 AND 4



FIGS. 5 AND 6. OBSERVED RELATIVE FREQUENCIES OF THE DIFFERENT JUDGMENTS OF *Ob* 5 AND 6



FIGS 7 AND 8. OBSERVED RELATIVE FREQUENCIES OF THE DIFFERENT JUDGMENTS OF *Os* 7 AND 8

the 'intermediate' curve. The curves for *O 5* (Fig. 5) are again all very regular. Those for *O 6* (Fig. 6) are relatively regular in form, as are also those for *O 7* (Fig. 7). In the 'intermediate' curve of *O 7* there is a slight inversion at the 92–96 grm interval. Finally the curves for *O 8* (Fig. 8) are only fairly regular. On the whole, however, the curves of all the *O*s show about as much regularity as those obtained from the usual comparative judgment series with this same material. Only 3 inversions are present in the 24 curves and, in every case, the inversion is very slight.

The numerical values for these results will be found in the upper halves of Tables I–VII. All the tables are similarly constructed and the complete results for every *O* will be found there. The results for the Absolute Series, which we will first consider, are in the upper half of every table and the results of the Relative Series, to be considered later, in the lower halves. In successive columns will be found the values of h_1 and h_2—the coefficients of precision of the 'light' and 'heavy'—judgments; S_1 and S_2—the lower and upper thresholds; the interval of uncertainty; and the point of subjective equality. In successive rows will be found these values for every fractionated series in the order in which they were obtained. The average values for the Absolute Series are in the sixth row.

For purposes of comparison of the *O*s, we shall first consider only the average values of the Absolute Series. For *O 1*, the values of h_1 and h_2 are relatively similar, that for h_1 being somewhat greater, indicating a somewhat higher precision of the 'light' judgments. The interval of uncertainty is large—the third largest of any of the 8 *O*s. The point of subjective equality is identical with the intermediate stimulus of the series. For *O 2*, the values of h_1 and h_2 are almost identical and that of the interval of uncertainty the third smallest for our *O*s. The point of subjective equality is exceedingly low—2 grm. lower than that for any other *O*. For *O 3* the values of h_1 and h_2 are almost identical, the value of the interval of uncertainty is next to the smallest among our *O*s, and the point of subjective equality stands intermediate among them. With *O 4*, the values of h_1 and h_2 are both high—that for the 'heavy' judgments showing slightly greater precision. For this *O*, the value of the interval of uncertainty is the lowest obtained—almost 2.5 grm. lower than for the next *O*—and the point of subjective equality ranks in the intermediate group. For *O 5* the values of h_1 and h_2 are both high and very similar, the value for the interval of uncertainty is intermediate, and the value for the point of subjective equality is next to the highest. The values of h_1 and h_2 for *O 6* are relatively low—the 'heavy' judgments showing relatively greater precision than the lighter; the interval of uncertainty is next to the highest obtained by our *O*s, and the point of subjective equality ranks among the intermediate values. For *O 7* the values of h_1 and h_2 are both low—the 'heavy' judgments showing the relatively greater precision; the value of his interval of uncertainty is the highest among our *O*s—being almost 3 grm. larger than the next highest; and his point of subjective equality might be char-

TABLE I

SHOWING RESULTS OF *0 1* IN THE ABSOLUTE (I) AND RELATIVE (II) SERIES OF EXPERIMENTS

(h_1 and h_2 designate respectively the coefficients of precision of the 'light' or 'lighter' and the 'heavy' or 'heavier' judgments; S_1 and S_2, the lower and upper thresholds; I. of U., the interval of uncertainty; and P.S.E., the point of subjective equality.)

Series	h_1	h_2	S_1	S_2	I. of U.	P.S.E.
I_1	0.096	0.086	88.02	100.35	12.33	93.84
I_2	0.081	0.078	87.17	102.66	15.49	94.79
I_3	0.094	0.077	87.54	103.68	16.14	94.81
I_4	0.071	0.082	87.92	106.38	18.46	97.83
I_5	0.071	0.062	88.76	110.11	21.35	98.76
Ave.	0.083	0.077	87.88	104.64	16.75	96.01
II_1	0.068	0.080	90.40	103.37	12.97	97.59
II_2	0.071	0.091	90.28	102.26	11.98	97.04
II_3	0.078	0.076	89.85	103.52	13.67	96.58
II_4	0.103	0.082	92.92	102.92	10.00	97.35
Ave.	0.080	0.082	90.84	103.02	12.16	97.14

TABLE II

RESULTS OF *0 2*

Series	h_1	h_2	S_1	S_2	I. of U.	P.S.E.
I_1	0.051	0.052	78.76	93.62	14.86	86.26
I_2	0.068	0.083	80.62	90.40	9.78	85.94
I_3	0.073	0.090	81.98	93.67	11.69	88.40
I_4	0.096	0.071	85.63	98.54	12.91	90.69
I_5	0.064	0.072	80.86	94.79	13.93	88.24
Ave.	0.070	0.074	81.57	94.20	12.63	87.91
II_1	0.083	0.079	91.69	94.43	2.74	93.23
II_2	0.085	0.082	89.55	91.25	1.70	90.38
II_3	0.103	0.111	89.59	93.05	3.46	91.38
Ave.	0.090	0.091	90.28	92.91	2.63	91.66

TABLE III

RESULTS OF *0 3*

Series	h_1	h_2	S_1	S_2	I. of U.	P.S.E.
I_1	0.061	0.089	86.89	97.11	10.22	92.96
I_2	0.063	0.067	85.09	97.71	12.62	91.54
I_3	0.076	0.083	86.52	98.71	12.19	92.85
I_4	0.111	0.086	86.96	100.55	13.59	92.89
I_5	0.093	0.077	85.90	94.83	8.93	89.94
Ave.	0.081	0.080	86.27	97.78	11.51	92.04
II_1	0.085	0.082	88.99	100.80	11.81	94.79
II_2	0.094	0.088	90.75	100.00	9.25	95.52
II_3	0.078	0.072	91.74	99.75	8.01	95.61
II_4	0.103	0.086	92.74	98.99	6.25	95.60
Ave.	0.090	0.082	91.06	99.88	8.83	95.38

TABLE IV

RESULTS OF *0 4*

Series	h_1	h_2	S_1	S_2	I. of U.	P.S.E.
I_1	0.115	0.103	86.94	97.00	10.06	91.70
I_2	0.122	0.118	86.98	95.56	8.58	91.20
I_3	0.091	0.104	86.17	96.06	9.89	91.46
I_4	0.111	0.156	89.29	97.47	8.18	94.07
I_5	0.109	0.169	89.12	97.84	8.72	94.42
Ave.	0.110	0.130	87.70	96.79	9.09	92.57
II_1	0.093	0.098	93.81	101.44	7.63	97.72
II_2	0.083	0.084	93.86	100.75	6.89	97.35
II_3	0.115	0.152	92.27	98.97	6.70	96.08
II_4	0.113	0.130	92.21	98.68	6.47	95.68
Ave.	0.101	0.116	93.04	99.96	6.92	96.71

TABLE V

RESULTS OF *0 5*

Series	h_1	h_2	S_1	S_2	I. of U.	P.S.E.
I_1	0.103	0.098	82.40	96.53	14.13	89.28
I_2	0.117	0.144	86.70	100.00	13.30	94.02
I_3	0.108	0.146	89.00	102.55	13.55	96.80
I_4	0.117	0.144	90.12	104.28	14.16	97.94
I_5	0.115	0.097	87.88	103.15	15.27	94.86
Ave.	0.114	0.126	87.22	101.30	14.08	94.58
II_1	0.124	0.110	92.65	99.46	6.81	95.86
II_2	0.123	0.140	94.42	97.77	3.35	96.21
II_3	0.116	0.125	93.06	97.20	4.14	95.20
II_4	0.104	0.134	94.23	98.00	3.77	96.35
Ave.	0.117	0.127	93.59	98.11	4.52	95.90

TABLE VI

RESULTS OF *0 6*

Series	h_1	h_2	S_1	S_2	I. of U.	P.S.E.
I_1	0.066	0.077	77.76	95.26	17.50	87.21
I_2	0.069	0.093	83.08	100.47	17.39	93.09
I_3	0.059	0.090	82.83	98.87	16.04	92.52
I_4	0.054	0.085	79.57	98.86	19.29	91.33
I_5	0.064	0.091	83.30	102.26	18.96	94.42
Ave.	0.062	0.087	81.31	99.14	17.84	91.71
II_1	0.092	0.109	91.47	100.75	9.28	96.52
II_2	0.069	0.125	91.48	103.06	11.58	99.00
II_3	0.073	0.094	90.93	100.75	9.82	96.36
II_4	0.095	0.087	92.00	97.14	5.14	95.01
Ave.	0.082	0.104	91.47	100.42	8.96	96.72

TABLE VII

RESULTS OF *O 7*

Series	h_1	h_2	S_1	S_2	I. of U.	P.S.E.
I_1	0.035	0.049	76.20	104.32	28.12	92.64
I_2	0.042	0.059	76.34	99.26	22.92	89.77
I_3	0.058	0.076	81.74	96.87	15.13	90.32
I_4	0.069	0.082	83.95	97.69	13.74	91.42
I_5	0.038	0.078	73.37	96.76	23.39	89.15
Ave.	0.048	0.069	78.32	98.98	20.66	90.66
II_1	0.050	0.066	84.61	100.62	16.01	93.80
II_2	0.066	0.095	87.70	100.09	12.39	94.97
II_3	0.064	0.084	88.30	99.27	10.97	94.43
II_4	0.166	0.141	87.28	96.31	9.03	91.43
Ave.	0.086	0.096	86.97	99.07	12.10	93.66

TABLE VIII

RESULTS OF *O 8*

Series	h_1	h_2	S_1	S_2	I. of U.	P.S.E.
I_1	0.074	0.053	84.43	101.44	17.01	91.51
I_2	0.048	0.060	78.21	99.98	21.77	90.33
I_3	0.060	0.066	80.06	96.15	16.09	88.51
I_4	0.095	0.065	85.87	97.42	11.55	90.57
I_5	0.067	0.075	83.19	98.03	14.84	91.00
Ave.	0.069	0.064	82.35	98.60	16.25	90.38
II_1	0.072	0.074	89.83	96.14	6.31	93.04
II_2	0.074	0.068	89.28	96.25	6.97	92.61
II_3	0.088	0.109	90.54	95.91	5.37	93.44
II_4	0.100	0.109	90.90	94.99	4.09	93.04
Ave.	0.084	0.090	90.14	95.82	5.68	93.03

acterized as low intermediate. Finally, for *O 8*, h_1 and h_2 are low but almost identical, the interval of uncertainty is intermediate, and the point of subjective equality is next to the lowest.

A consideration of the values of the indices of precision shows all of them to be the usual magnitude that one finds for the generally used comparison series. This indicates that these absolute judgments of 'light' and 'heavy,' made without any standard stimulus, may have as great precision as one obtains in the general practice of presenting standard and comparison stimuli alternately. For *Os 2*, *3*, and *8*, the values of the two indices of precision are practically identical. For *O 1* alone is the index for the 'light' judgments greater than that for the 'heavy.' For *Os 4*, *5*, *6*, and *7* the 'heavy' judgments are given with greater precision than the light.

It is in the matter of the size of the intervals of uncertainty that the present Absolute Series differ from the usual Relative Series, inasmuch as the values are considerably larger for the Absolute Series. These values also show great variability from one *O* to another, ranging from a very little more than 9 grm. to almost 21 grm. On the whole, these values are somewhat larger than those obtained by Wever and Zener[4] but the two series from the two experiments are quite comparable.

Inasmuch as the size of the interval of uncertainty is directly dependent upon the number of judgments given in any intermediate category or categories, the increased size of this interval for our absolute series indicates that relatively more 'intermediate' judgments were given in this series than are 'equal' judgments usually given in the normal relative series. At a perfectly common sense level it would seem that this result should be expected. The ideal would be to have 100% of 'intermediate' judgments on the one or several intermediate stimuli. This orientation towards this situation is so unusual that it is difficult to say what the ideal would be without further experimentation. The rank order of the size of the interval of uncertainty from lowest to highest for our *O*s is *4*, *3*, *2*, *5*, *8*, *1*, *6*, and *7*.

The values of the points of subjective equality for our Absolute Series are all either identical with or lower than 96 grm. which was the middle weight of the series. The calculated value for *O 1* is identical with the central value of stimulus—all of the rest range from more than 1 grm. to more than 8 grm. lower. The rank order of the *O*s from the lowest to the lightest is *2*, *8*, *7*, *6*, *3*, *4*, *5*, and *1*. It will be observed that this divergence of the calculated point of subjective equality is in every case in the direction of what has been considered the effect of the time error in the normal comparison series. The results exactly conform to those obtained by Wever and Zener noted above, although the magnitude of the divergence of our results is greater than theirs. These authors conclude that "it would seem that the phenomenon goes beyond the mere matter of the temporal order of standard and comparison."[5] This question will be discussed more fully later.

Effects of progressive practice. It seems worth while to study our fractionated data for every *O* to determine the possible effects of

[4]Cf. Wever and Zener, *op. cit.*, esp. table on p. 482.
[5]Cf. Wever and Zener, *op. cit.*, 488.

progressive practice in the Absolute Series. What has been found to happen in the normal comparison series as a result of progressive practice is well known for lifted weights. Urban has shown that progressive practice increases the size of the values of the coefficients of precision; decreases the interval of uncertainty; and, to a less degree, lowers the point of subjective equality;[6] and furthermore that these effects follow the form of the usual practice curve, being greater at first and gradually diminishing in effect. Fernberger has shown that these effects in the normal relative lifted-weight series are very great at the initial stages of practice.[7]

The effects of practice in the Absolute Series on the coefficients of precision may be outlined as follows: *O 1* shows a decrease in both; *O 2*, an increase in both; *O 3*, an increase in both; *O 4*, little change for that of the 'light' judgments and an increase for the 'heavy' judgments; *O 5*, little change for a slight increase for the 'light' and an increase for the 'heavy' judgments; *O 6*, little change for the 'light' and an increase for the 'heavy' judgments; and *Os 7* and *8*, increases for both. In other words, except for *O 1*, the tendency for the effect of progressive practice is similar for the Absolute Series and the normal relative series—in both cases tending to increase the size of the coefficients of precision. In the present study for the Absolute Series, this tendency is more marked for the 'heavy' than for the 'light' judgments. There was greater precision of 'heavy' judgments, as the result of practice, for 7 of the 8 *Os*.

With regard to the size of the interval of uncertainty the results may be briefly summarized. *Os 4*, *7*, and *8* show a marked decrease in the size of the interval of uncertainty as the result of practice, and *O 2* a somewhat smaller tendency in the same direction. In other words 4 of the 8 *Os* give results which conform to the usual Relative Series in this respect. In the case of these *Os* the decrease is perfectly steady. *O 3* shows at first an anomalous initial increase followed by a decrease in the size of the interval of uncertainty. *Os 5* and *6* show an initial normal decrease followed by an increase in the magnitude of this interval which is, in the case of *O 5*, only slight. Although there are inversions, all these *Os* (*3*, *5*, and *6*) give results that, on the whole, are in conformity with the usual practice effects on the size of the interval of uncertainty. *O 1* again gives completely anomalous results—the size of the interval of uncertainty steadily increasing in magnitude from series to series.

With regard to the effect of progressive practice on the point of subjective equality for the Absolute Series, *5 Os* (*1*, *2*, *4*, *5*, and *6*) show the normal effect of an increase in the magnitude of this value so that it more nearly approaches the central value of the series. For *Os 3* and *8* there is little or no change in these values. The results for *O 7* are anomalous, showing a slight decrease in the size of the point of subjective equality.

[6] F. M. Urban, Der Einfluss der Übung bei Gewichtsversuchen, *Arch. f. d. ges. Psychol.*, 29, 1913, 271-311.

[7] S. W. Fernberger, The effects of practice in its initial stages in lifted-weight experiments and its bearing upon anthropometric measurements, this JOURNAL, 27, 1916, 261-272.

TABLE IX

SHOWING AVERAGES OF THE RESULTS OF ALL THE *Os* IN THE ABSOLUTE SERIES FOR EVERY ONE OF THE FIVE PARTS INTO WHICH THE DATA WERE FRACTIONATED

Series	h_1	h_2	I. of U.	P.S.E.
I_1	0.075	0.076	15.28	90.68
I_2	0.076	0.088	15.23	91.34
I_3	0.077	0.092	13.80	91.96
I_4	0.090	0.096	13.98	93.34
I_5	0.078	0.090	15.67	92.60

The averages for all of these results for all 8 *Os* for every one of the 5 parts into which the results were fractionated are to be found in Table IX. Certain inversions are evident in these average results but on the whole progressive practice seems to have exactly the same effects on the Absolute Series as in the normal relative series for lifted-weights. The values of the coefficients of precision for both the 'light' and 'heavy' judgments show a tendency to increase; the values of the interval of uncertainty show a tendency to decrease in spite of the anomalous value for the last series; and the values of the point of subjective equality show a tendency to increase.

Series II (*Relative Series*). The average observed relative frequencies for the Relative Series which were given to the *Os* after the Absolute Series were completed will be found in Fig. 1–8, indicated by the broken lines. In every case the curves of the psychometric functions are relatively of the normal ogive form. Very few inversions are to be noted: a slight inversion for 'equal,' 108 for *O 2*; one very slight for 'heavier,' 92 for *O 6*; and a very slight inversion for 'heavier,' 92 for *O 8*. It will be observed that in every case there is a tendency for all of the curves to shift to the right toward the 'higher' values of stimulus as compared with those of the Absolute Series. The significant values of these Relative Series will be found in the lower halves of Tables I–VIII. One may readily compare these results with those obtained by every *O* in the Absolute Series.

The values of the coefficients of precision of the 'lighter' judgments indicate that the values for the Relative Series are much larger for *Os 7* and *8* than for the Absolute Series; are somewhat larger for *Os 2*, *3*, and *6*; are approximately equal for *Os 1* and *5*; and slightly smaller for *O 4*. The coefficients for the 'heavier' judgments are much larger in the Relative Series for *Os 2*, *7*, and *8*, somewhat larger for *Os 1* and *6*, equal for *Os 3* and *5*, and slightly smaller for *O 4*. The general picture, then, is one of greater precision for both the 'lighter' and 'heavier' judgments for the Relative Series than for the Absolute Series.

For every one of the 8 *Os* the lower threshold (S_1) is higher for the Relative Series than for the Absolute Series. With regard to the upper threshold (S_2) the values for the Relative Series are smaller than those for the Absolute Series for *Os 1*, *2*, *5*, and *8*; approximately equal for *O 7*; and only slightly larger for *3*, *4*,

and *6*. There is a general tendency, then, for the upper and lower thresholds to more nearly approach each other in the Relative than in the Absolute Series.

This situation becomes obvious when one compares the size of the intervals of uncertainty or the difference between the two thresholds. For every one of the 8 *O*s, the size of the interval of uncertainty is smaller in the Relative than for the Absolute Series. In rank order the differences are greatest for *O 8*, and in decreasing order follow *O*s *2*, *5*, *6*, *7*, *1*, *3*, and *4*. In no case is the difference smaller than 2 grm. (*O 4*), and in one case this difference is greater than 10.5 grm. (*O 8*)

Although the size of the intervals of uncertainty is smaller for the Relative than for the Absolute Series, still on the whole it is larger than one usually obtains under identical conditions for a Relative

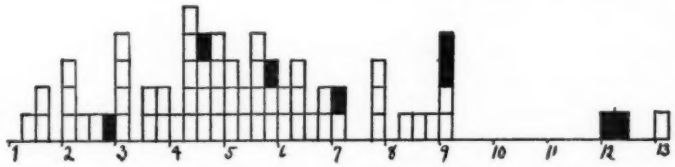


FIG. 9. DISTRIBUTION OF INTERVALS OF UNCERTAINTY
Cases from earlier experiments in 'white,' from the present experiment in 'black.'

Series. Some time ago, and for another purpose, we listed the size of the intervals of uncertainty obtained from 58 *O*s under identical conditions of the Relative Series.[8] This chart is reproduced as Fig. 9 with the addition of the present eight intervals of uncertainty in solid black. It will be observed that 4 of the present 8 *O*s give intervals larger than all but one of the previous 58; and that 6 *O*s of the present 8 are well above the median value of the former curve—indeed all of these 6 are above the upper 70% point of the curve.

With regard to the position of the points of subjective equality of the two series of experiments, these points are invariably higher for every *O* in the Relative Series than in the Absolute.

There is variability of this difference between the two points of subjective equality from *O* to *O*. The smallest difference is about 1.5 grm. for *O 5* and the largest is approximately 5 grm. for *O 6*. For the other 6 *O*s the range is small varying from 3 to 4 grm. The average for all *O*s is almost exactly 4 grm. May the fact again be recalled that the central value of the Absolute Series was 96 grm. and the standard weight for the Relative Series was 100 grm.—just the 4 grm. difference in the right direction!

[8] S. W. Fernberger, An unconsidered source of material for the problem of individual differences, this JOURNAL, 42, 1930, 646-647.

Effects of progressive practice. In spite of the fact that every *O* made 1750 absolute judgments before starting the Relative Series, the effects of practice are evident in this series. The averages for all *O*s are given for every fractionated part in Table X. In the case of

TABLE X

SHOWING AVERAGES OF THE RESULTS OF ALL THE *O*s IN THE RELATIVE SERIES FOR EVERY ONE OF THE FOUR PARTS INTO WHICH THE DATA WERE FRACTIONATED

Series	h_1	h_2	I. of U.	P.S.E.
II_1	0.083	0.087	9.20	95.32
II_2	0.083	0.097	8.01	95.38
II_3	0.089	0.103	7.77	94.88
II_4	0.112	0.110	6.39	94.92

the coefficient of precision of the 'lighter' judgments (h_1) there is a regular average increase. This is also evident for the coefficient of precision of the 'heavier' judgments (h_2) as one proceeds from part to part in the order in which they were obtained. There is a regular decrease of considerable magnitude of the size of the interval of uncertainty. All of these effects are the usual result of progressive practice and were evident also for the Absolute Series as already noted above. The average values of the points of subjective equality show little change but there is a slightly evident tendency of a decrease in magnitude.

The results for the individual *O*s are not so regular as the averages. For the coefficient of precision of the 'lighter' judgments, only one *O* (*O 5*) gave results which did not conform to the average. For the coefficients of the 'heavier' judgments, 2 *O*s (*O*s *1* and *6*) failed to conform to the averages. For the size of the interval of uncertainty, only one *O* (*O 2*) failed to show a decrease in accordance with the usual effects of practice and it will be observed that that *O* gave a very small interval of uncertainty throughout for the Relative Series. For the point of subjective equality there is a tendency of increase for one *O* (*O 3*), no change for 3 *O*s (*O*s *1*, *5*, and *8*), and a tendency of decrease for the others (*O*s *2*, *4*, *6*, and *7*). One may safely say, therefore, that the results of the individual *O*s give a perfectly normal picture of the usual effects of progressive practice.

DISCUSSION

One of the outstanding findings of the present experimental study is the obtaining of the shift of the point of subjective equality in the Absolute Series entirely comparable with that in the Relative Series and in the same direction. Such a shift in the present procedure of the Relative Series has been ascribed to the time error. In this respect, our results completely confirm those of Wever and Zerner mentioned above.[9] These authors seem to favor the "sedi-

[9]Wever and Zener, *op. cit.*

ment" theory suggested by Köhler.[10] This theory presents certain highly acceptable features. There are other aspects of the present experiment, however, which suggest the possibility of another interpretation.

It will be remembered that for our Absolute Series, we chose *O*s who were completely naïve and unpracticed and that we never allowed these *O*s to lift a standard weight until the Absolute Series were entirely completed. Under these conditions we obtained occasional introspective reports, and discovered that all of the *O*s were, from time to time at least, employing a sort of intraserial relative judgment in the Absolute Series. For example, if an absolutely heavy weight were followed by one lighter—but not too much lighter—this second weight was likely to be judged 'intermediate.' On the other hand, if a weight just judged 'intermediate' were followed by one which was lighter it was almost sure to be judged 'light.'

Under these conditions of judging, it is obvious that the *O* might develop a muscular set with which he approached each successive weight of the sort described by Martin and Müller for the relative judgments.[11] In this case it seems reasonable to suppose that the same factors influencing this muscular set for the Relative Series would also be operative for the Absolute Series. The author is not convinced that the explanations of the basis of this error which have been propounded are completely satisfactory, but it seems reasonable to proceed on the basis of the older theory before propounding a new one.

The present study also completely confirms the results of Wever and Zener with regard to the greater magnitude of the interval of uncertainty for the Absolute Series. This is dependent upon the giving of relatively many more 'intermediate' judgments in this series than 'equal' judgments in the Relative Series. The author would accept their conclusion, within limits, that the number of 'intermediate' judgments may be controlled by the instructions and, indeed this was accomplished with marked success by Gahagan.[12] The author has recently shown that the number of 'equality' judgments may be controlled within very wide limits by instructions for

[10]W. Köhler, Zur Theorie des Sukzessivvergleichs und der Zeitfehler, *Psychol. Forsch.*, 4, 1923, 115-175.

[11]L. J. Martin and G. E. Müller, Zur Analyse der Unterschiedsempfindlichkeit, 1899. Cf. also S. W. Fernberger, An introspective analysis of the process of comparing, *Psychol. Monog.*, 24, 1919, (no. 117), 1-161.

[12]Gahagan, *op. cit.*

the Relative Series.[13] The results of many experiments indicate that these instructions form different attitudes in the *Os*.

Some years ago, the author in a meeting spoke of the development of a "mental standard" in such psychophysical experiments and he was severely taken to task by Titchener. After a number of years, he has come back to this concept in a modified form. This idea is capable of description in several very different ways. The "mental standard" may be nothing but the "muscular set" of Martin and Müller and so would not be strictly "mental" at all. It would seem that this factor may enter into it but that the "standard" involves much more. It involves most certainly the *O*'s definition of his categories of judgment. Köhler would conceive this as the formation of *Gestalten* of the categories—a point of view which may still be unable to completely explain the present problem but one which, we believe, is much more than a mere substitution of terms.[14]

According to this point of view, progressive practice would consist very largely in the building up of definitions of the categories of judgment. A comparison of the rank order of the size of the intervals of uncertainty of the Absolute and the Relative Series shows a general but by no means a marked correlation for every one of the *Os*. The actual rank order, from lowest to highest, are: Absolute Series, *Os 2, 5, 8, 4, 3, 6, 7, 1;* Relative Series, *Os 4, 3, 2, 5, 8, 1, 6, 7*. The coefficient of correlation, which is derived from too few cases to have statistical validity but which may give an indication, is 0.48. On the other hand, the 5 *Os* with the lowest intervals of the Absolute Series are also lowest in the Relative Series but in different order. And again, in different order, the remaining *Os* (*1, 6,* and *7*) have the largest intervals of uncertainty for both the Absolute and Relative Series. In other words, here is some evidence that the definition of the intermediate category of the Absolute Series—of including relative many or few intermediate judgments—carries over to a certain extent to the Relative Series as well.

But there seems little doubt that the highly developed definition of the 'intermediate' category for the Absolute Series was different from that required for the Relative Series. The evidence of this is that perfectly normal effects of progressive practice are evident for the Relative Series in spite of the long practice in 'absolute' judgments which has just preceded.

[13] S. W. Fernberger, Instructions and the psychophysical limen, this JOURNAL, 43, 1931, 361-376.

[14] Cf. W. Köhler, *Gestalt Psychology*, 1929, Chap. VI.

Conclusions

(1) If completely naïve *O*s are started with 'absolute' judgments of a series of lifted-weights in which no standard weight is ever presented, one obtains normal curves of the psychometric functions as the basis of the calculation of limens.

(2) The normal effects of progressive practice are evident in the Absolute Series which are shown as an increasing precision of judgment and as a decreasing size of the interval of uncertainty.

(3) There is a lowering of the point of subjective equality below the central value of the Absolute Series of about the same magnitude which one usually obtains in the Relative Series and which has there been explained in terms of the time error.

(4) As compared with the subsequent Relative Series, the significant values of the Absolute Series vary in the following ways: (a) there is less precision of judgment, and (b) the intervals of uncertainty are much greater for the Absolute Series.

(5) In spite of the training in 'absolute' judgments, there is an evident effect of normal progressive practice for the subsequent Relative Series.

(6) The view is here suggested that progressive practice largely consists in the increasingly adequate definition of the categories of judgment. If this be true, some effect of the development of this definition carried over from the Absolute to the Relative Series, as evidenced by the rank orders of the *O*s in respect to the size of the intervals of uncertainty in the two series and also because these intervals for the Relative Series were larger than one usually obtains with the same experimental arrangement. On the other hand, the fact that normal progressive practice is evident in the subsequent Relative Series, is evidence that the definitions of the categories of judgment in the Absolute Series could not be carried over entirely and without change.

MEANINGFUL RELATION AND RETROACTIVE INHIBITION

By JOHN A. McGEOCH, University of Missouri, and WILLIAM T. McDONALD, University of Chicago

The relationships which exist between the degree of retention of memorized material and the kind (or amount) of the activity interpolated between memorization and the measurement of retention constitute one of the most urgent problems of the psychology of learning. Involved in the problem are both the important factual issue of the correlations between retention and interpolated activity and the equally significant theoretical question of the factors to which retroactive inhibition is to be referred.

The theoretical question has been an active one since the original work of Müller and Pilzecker,[1] and two major theories have been framed. According to one, the perseveration theory, inhibition is to be ascribed to the interference offered by interpolated activity to the perseveration or setting-in of the original lists. According to the other, the transfer theory, inhibition results from a confusion between the items of the original and interpolated lists, or from some form of blocking of the former by the latter. Inhibition should, therefore, vary with the similarity obtaining between the two lists. In the light of this theory, Skaggs[2] and Robinson[3] have predicted that, were the similarity to be varied on a continuous scale from identity to zero or very low similarity, the curve of retroaction would rise from zero retroaction when the original and interpolated materials are identical to a high point somewhere between identity and very great dissimilarity, and then fall to a small amount of inhibition at the latter point.[4]

*Accepted for publication November 20, 1930. The experimental work reported here was done in the psychological laboratory of the University of Arkansas.

[1]G. E. Müller and A. Pilzecker, Experimentelle Beiträge zur Lehre vom Gedächtniss, *Zsch. f. Psychol.*, Erg. Bd. 1, 1900, 1-300.

[2]E. B. Skaggs, Further studies in retroactive inhibition, *Psychol. Monog.*, 34, 1925, (no. 161), 1–60.

[3]E. S. Robinson, The 'similarity' factor in retroaction, this JOURNAL, 39, 1927, 297-312.

[4]The work which has been done upon theories of retroactive inhibition and upon the Skaggs-Robinson hypothesis will be found summarized in E. S. Robinson, Some factors determining the degree of retroactive inhibition, *Psychol. Monog.*, 28, 1920, (no. 128), 1-57; Memory, *Psychol. Bull.*, 21, 1924, 569-594; J. A. McGeoch, Memory, *Psychol. Bull.*, 25, 1928, 513-549; 27, 1930, 514-563.

The present experiments have been designed to determine the degrees of retroactive inhibition resulting from the interpolation of materials which differ widely in their closeness of meaningful relation to the original lists. The results will serve as one test of the transfer theory of retroaction, and will show whether, with these materials, the Skaggs-Robinson prediction can be verified. In preceding investigations similarity has been rated by the judgment of the individual experimenter, and interpolations differing widely in both form and content from the original lists have been employed without regard for meaningful relations, or else the similarity has been more directly quantified in terms of degree of formal identity. In the normal forgetting of everyday acquisitions the meaningful relations of original and interpolated materials appear to be of more importance than formal identity on account of their much greater frequency of occurrence. We have sought to deal with such relations, and to determine the degrees of relation between our materials in terms of the ratings of a large number of judges. Our method is admittedly rough and exploratory and far short of a rigid quantification, but there seems no reason why the outcome should not be valid in tendency.

Experiment I

Technique. In Experiment I, as also in Experiment II to be described later, the customary technique of the studies of retroactive inhibition was employed. Retention after an interval of rest (no interpolated learning activity) is used as a standard with which is compared the retention after an equal interval of work (an interpolated learning activity). Retroactive inhibition is measured in terms of the difference between the amounts retained under these two conditions.

Method and procedure. Six conditions, 1 rest and 5 work, were compared. The original learning materials consisted throughout of lists of 11 two-syllable adjectives. The interpolated materials, which were introduced immediately following the last presentation of the original list, were: (1) a list of adjectives each of which was a synonym of the adjective in the corresponding position in the original list; (2) a list of adjectives each of which was an antonym of the adjective in the corresponding position of the original list; (3) a list of adjectives unrelated to the original list; (4) a list of nonsense syllables chosen from Glaze's 0% classification;[5] and (5) a list of 3-place numbers. The rank-order of these 5 materials with respect to their relation to the original lists was determined by the ratings of 70

[5] J. A. Glaze, The association value of nonsense syllables, *Ped. Sem.*, 35, 1928, 255-267.

undergraduates from whom the *S*s had been chosen and of 10 graduate students and faculty members. The judges were asked first to rank the materials and then to place each of them on a scale of 100 with reference to their degree of closeness of relation, as learning materials, to a list of adjectives whose duplication would represent the 100% point. The mean of the ratings of each material was found and this figure used to indicate roughly the distance apart of the five materials with respect to their relation to the original list. The order follows that in which they are listed in the description of the interpolated conditions. The distances apart are represented by the following values, assigned to the 5 materials on a scale of 100: synonyms, 92.6; antonyms, 56.3; unrelated adjectives, 29.5; nonsense syllables, 17.0; and 3-place numbers, 11.6. A more precise method of scaling might be applied, but the order is clearly established and exact scaling is for the present purpose unnecessary. It may at first seem surprising that antonyms should be judged to be more related to the adjectives of which they are antonyms than are two lists with no shade of opposite meaning involved. It must be remembered, however, that the judges were instructed to rate in terms of the relatedness of the lists as learning materials. Save for a few judges antonyms were placed between synonyms and unrelated adjectives, presumably in the opinion that a synonymous relation is more obvious than an antonymous one, and that the latter is well above the general relation of verbal character as adjective, as represented in the list of unrelated adjectives. The independent ratings of 10 graduate students and faculty members agrees closely with the undergraduate values.

Twelve *S*s went through the 6 conditions in a counterbalanced practice order. The lists were exposed at a 2-sec. rate on an electrically driven memory drum and were learned by the anticipation method. By this method the first item serves as a cue and is not learned, so that our lists are uniformly 10 items in length for the purposes of learning. The adjectives were anticipated by pronouncing, the syllables by spelling, and the numbers by reciting in order each succeeding item.[6] A constant frequency of 5 trials was employed with the original lists and one of 10 trials with the interpolated lists. These frequencies are such as to yield degrees of retroaction large enough to permit a clear differentiation between the influences of the different interpolations to show itself if it exists. An interval of 10 min. was permitted to elapse between the last presentation of the original list and the beginning of relearning. After this interval the original list was relearned to a criterion of three successive perfect trials, and the interpolated list was given one relearning presentation to guard against an impression on the

[6]This difference in method of anticipating is almost unavoidable. To insist that the *S*s spell the adjectives would introduce an unusual and disturbing condition, while to ask that they pronounce the syllables would render it difficult for the *E* to determine exactly whether the *S* had really made a correct anticipation on account of the almost unpronounceable character of some syllables. The numbers could scarcely be anticipated in any other way than by serial recitation. Mr. A. W. Melton, of the Pennsylvania Museum of Art, who has been kind enough to read the manuscript, has suggested that these differences in method of anticipation may introduce a changed *Aufgabe* which enhances the dissimilarity of the syllables and numbers. This is possible. It can scarcely have done more, however, than accentuate the small amounts of inhibition yielded by the syllables and numbers by making these amounts still smaller.

part of *S* that the interpolated lists were not to be learned with the intent to retain. Under the rest condition the 10-min. interval was occupied by reading *College Humor* under instruction to select and mark the three best jokes on each page.

Four different measures of retention have been taken: (1) Recall 1, the number of words correctly anticipated on the first relearning trial; (2) Recall 2, the number correctly anticipated on the second relearning trial; (3) Relearning 1, the number of trials required to relearn to one perfect repetition; and (4) Relearning 2, the number of trials required to relearn to three successive perfect trials.[7]

In evaluating the results it is necessary to know first whether the 6 original lists have been learned to an approximately equal degree in the 5 repetitions. A fair measure of degree of learning is yielded by the total number of correct anticipations. The mean anticipations per *S* for the rest condition and the 5 work conditions, in the order listed above, are: 18.5, 15.9, 19.8, 16.8, 18.3, and 20.5. The differences between the means do not run in any regular order, and no difference between any two means is as much as 3 times its sigma. We may, then, consider whatever differences may appear between the retention scores under the experimental conditions to be a function of other factors than degree of learning.

Results. The means of the recall and relearning scores, with their standard errors, are presented in Table I. Percentages of retroactive inhibition will not be given since the meaning of the data is sufficiently

TABLE I

MEAN RECALL AND RELEARNING SCORES

Interpolation	Recall 1		Recall 2		Relearning 1		Relearning 2	
	Mean	Sigma	Mean	Sigma	Mean	Sigma	Mean	Sigma
Rest	4.50	.83	6.50	.63	4.58	.66	5.17	.72
Synonyms	1.25	.49	4.17	.65	7.33	.91	9.08	1.34
Antonyms	1.83	.55	4.42	.45	6.67	.80	7.00	.89
Unrel. Adj.	2.17	.70	5.00	.75	5.17	.59	6.67	.73
Syllables	2.58	.64	5.42	.38	4.50	.46	7.17	1.02
3-pl. no.	3.68	.72	7.58	.68	4.42	.53	5.08	.56

clear from the means themselves. In terms of both measures of recall, retroaction decreases consistently from more to less similarity, with a sharp drop between syllables and 3-place numbers. In terms of the two measures of relearning, inhibition decreases with de-

[7]The most important measures are Recall 1 and Relearning 2, since the former represents the amount recalled before an opportunity has been given for any relearning, while the latter is computed in terms of the more rigorous criterion. Recall 2 has been included, however, since it provides one measure of initial progress in relearning. Relearning 1 has been computed to ascertain whether the data for relearning to a less rigorous criterion follow those for the more rigorous one.

creasing similarity save for a slight and statistically unimportant inversion in Relearning 2 when syllables are the interpolated material. Recall 1 after the learning of interpolated numbers is still some distance below the corresponding score after rest, while Recall 2 is higher after number-learning than after rest. The numbers of trials taken to relearn under these two conditions are very nearly the same. Since inhibition virtually disappears when 3-place numbers are interpolated, it seems that similarity has been carried well toward the point at which it ceases to have an inhibitory effect.

Before we can accept the conclusions stated with any certainty it is necessary to inquire regarding the degree of learning of the different interpolated materials.[8] While it is not certainly known that inhibition varies directly with degree of interpolated learning, it is a fair assumption that it might do so since inhibition does vary inversely as the degree of learning of the original lists.[9] In any case, should there be a direct correspondence between amount of inhibition and degree of interpolated learning, the possibility would be apparent that the different materials may have influenced inhibition only through their varying difficulty for learning. The mean numbers of correct anticipations are: synonyms, 58.1; antonyms, 57.1; unrelated adjectives, 67.2; nonsense syllables, 30.8; and 3-place numbers, 29.2. Synonyms and antonyms have been learned to about the same degree and unrelated adjectives have been better learned than either.[10] Syllables and numbers have been equally learned, but to a smaller degree.

These materials produce, in the order listed, a steadily decreasing amount of retroactive inhibition. The conclusion that this decreasing inhibition is a function of the character of the materials and not of decreasing degrees of learning is indicated. The very much smaller number of correct anticipations for interpolated syllables and numbers, however, raises the question whether the low amount of inhibition which they yield may validly be attributed to the nature of the

[8]It is important to note that, in prior studies of the similarity factor, no account has been taken of degrees of learning of either original or interpolated materials. In certain of the investigations the conditions have been such as to lead one to expect widely differing degrees of learning among the conditions compared.

[9]J. A. McGeoch, The influence of degree of learning upon retroactive inhibition, this JOURNAL, 41, 1929, 252-262.

[10]It is an interesting question whether the fewer correct anticipations for synonyms and antonyms than for unrelated adjectives is indicative of a valid proactive inhibition.

material independently of the degree of learning. Two considerations weigh in favor of an affirmative answer. (1) The three groups of interpolated adjectives, rated by judges as possessing decreasing relation to their original lists, yield a steady decrease in retroaction in spite of the fact that the degree of learning is somewhat greater for the third group than for the two others. An extrapolation of this independence of character of material and degree of learning downward on the scale seems reasonable. (2) This becomes the more permissible in the light of the fact that, in spite of the slight difference in degree of learning between syllables and numbers, they are some distance apart in degree of retroaction as measured by recall scores and by Relearning 2. That the steady decrease in retroaction from synonyms to numbers is a function of the character of the materials, independently of degree of learning may be concluded. Whether, in the case of unrelated adjectives, syllables and numbers, degree of learning has greatly affected the absolute amounts of retroaction cannot be said and can be discovered only by further investigation. Were we to bring the degree of learning of interpolated syllables and numbers to an equality with that of the lists of adjectives, a much greater number of presentations of the two first would be required, and the interpretation of the results would be in no more certain condition, since it is not known whether differences in time spent in learning (here number of presentations) are a factor affecting retroaction independently of degree of actual learning. It is, however, a fair inference that syllables and 3-place numbers, as materials, produce less inhibition than do the three groups of adjectives and that this amount is a relatively small one.

One can conclude from the data presented that the learning of interpolated lists of synonyms, antonyms, unrelated adjectives, nonsense syllables, and 3-place numbers produces retroactive inhibition in amounts which decrease steadily from synonyms to numbers, until with the latter material inhibition becomes very small. No sign of the inversion demanded by the Skaggs-Robinson hypothesis appears.[11] It may be argued, however, that we did not employ degrees of similarity which approach sufficiently close to identity and that our results apply consequently only to the right section of the

[11]The curve which would result from the plotting of the tabular data can be envisaged readily in terms of the abscissa values already mentioned (mean ratings on a scale of 100), and it will not be shown here.

abscissa of the hypothetical curve. It was in order to meet this possible criticism that Experiment II was undertaken.

EXPERIMENT II

We tried in Experiment II to attain a closer approximation to identity of meaning than we had attained in Experiment I.

Method and procedure. An attempt was made to work with similarity near the identity end of the scale by subdividing synonyms into 3 groups with respect to their degree of synonymy. Sixty pairs of synonymous 2-syllable adjectives, mimeographed on a single sheet, were presented to each of the 80 judges already mentioned, who were asked to divide the pairs into three groups of 20 each with reference to their closeness of relation. The 10 pairs of synonyms located in each group most uniformly and consistently by the judges were selected as the learning materials. There were available, thus, three lists of 10 pairs of synonyms each. Each list of pairs was clearly separated from each other list by the judges' ratings. We may then consider these 3 groups of pairs to represent 3 degrees of closeness of synonymous relation, reaching from very close similarity of meaning to a much less close similarity. The pairs having the first degree of relation are, to most judges, identical in meaning.

The 4 conditions of the experiment were a rest condition, during which the interval was filled by conversation between *E* and *S*,[12] and 3 work conditions. In these work conditions a list composed of the first members of the pairs of a group served as the list for original learning, and a list composed of the corresponding second members was used for the interpolated learning. We shall designate the 3 work conditions by the numbers 1, 2, and 3 to represent the 3 degrees of similarity obtaining between the corresponding members of the original and interpolated lists.

Twenty-four college students went through the 4 conditions in a counterbalanced practice order. The conditions were identical with those of Experiment I.

In order to ascertain whether differences in degree of difficulty of the original lists can have affected the results by yielding differing degrees of learning, we shall present the numbers of correct anticipations during the 5 learning trials. The means of these numbers are, for the 4 conditions, respectively, 21.5, 23.0, 22.8 and 24.3. These differences are irregular, small and statistically unimportant. We may, then, consider whatever differences appear between the four conditions to be a function of other factors than degree of learning.

Results. The means of the recall and relearning scores, with their standard errors are presented in Table II. In terms of all measures of retention the rest condition is superior to each of the work conditions, which means that all measures show susceptibility to retroactive in-

[12]Conversation filled the rest interval of this experiment, instead of reading from *College Humor*, because many of the prospective *Ss* had learned the purpose of the reading, and it was judged a more perfect method of insuring against rehearsal to engage the *S* in conversation. In so far as can be told from this and other experiments, the two rest conditions are nearly equivalent.

hibition. This inhibition decreases consistently from the first to the third work condition in terms of Recalls 1 and 2, and of Relearning 2. Relearning 1 yields more inhibition at the second condition than at any of the others. This is statistically unimportant and may be considered a minor exception. It is clear that by the most significant measures of retention retroaction decreases consistently as the closeness of relation between the original and interpolated materials decreases. The relation, when plotted, is very nearly linear if we assume

TABLE II

RECALL AND RELEARNING SCORES

Condition	Recall 1		Recall 2		Relearning 1		Relearning 2	
	Mean	Sigma	Mean	Sigma	Mean	Sigma	Mean	Sigma
Rest	5.42	.44	7.46	.36	3.33	.42	3.67	.48
Work 1	0.83	.24	4.58	.33	4.83	.96	7.38	1.20
Work 2	1.33	.36	5.54	.32	5.21	.61	6.54	.86
Work 3	2.04	.37	6.75	.38	4.17	.43	4.50	.48

that the three work conditions are equal distances apart. The general fact of decreasing retroaction with decreasing similarity is not, of course, affected by the truth or falsity of this assumption.

In this experiment interpretation is uncomplicated by differences in degree of interpolated learning. The mean numbers of correct anticipations, for the three groups of adjectives, are 60.2, 57.8 and 58.8, respectively. The differences are neither uniform in direction nor of statistical consequence.

DISCUSSION

It is the outcome of these experiments (1) that retroaction decreases steadily and consistently from a high degree to a much lower one as synonyms, antonyms, unrelated adjectives, nonsense syllables, and 3-place numbers are interpolated; and (2) that the same decrease in retroaction appears when the interpolated lists are composed of synonyms in three different degrees of closeness of relation to their original lists. These statements describe the factual outcome, which is independent of any interpretation which may be made with reference to similarity. As they stand the results state the relative influence of different interpolated materials upon the retention of lists of adjectives.

It is important, however, to seek further and to inquire what bearing the facts may have upon theories of retroactive inhibition. To any observer, doubtless, it is evident that the interpolated materials

differ widely in their relations to lists of adjectives. In order to ascertain their rank-order with regard to this and to quantify the relationships, the ratings previously described were secured. These ratings are obviously rough, but it is, nevertheless, clear that they represent some factor to which retroactive inhibition bears a consistent relation. This relation and the ratings are necessarily independent, since the *S*s were entirely ignorant of the experimental results. The factor mentioned may fairly be designated as meaningful relation, since it was with respect to this that the judges were instructed to rate the materials. It follows that a transfer theory of retroaction is positively supported, since the possibilities of transfer may be expected to vary with degree of meaningful relation. There appear few possibilities of an interpretation in terms of the perseveration theory, inasmuch as the only continuous variable is the character of the materials interpolated and the degree of their relation to the original lists, and there are no materials differing in rated closeness of relation which yet produce equal degrees of retroaction.

Regardless of the precise distances apart of the materials, it is clear that there is no sign of the Skaggs-Robinson curve. The data of Experiment I resemble the portion of that curve stretching from the area of medial similarity toward zero. The findings of Experiment II, in one condition of which, at least, degree of synonymy is very high, fail to show any hint of an inversion. It may be that synonymy, with its inevitable divergence in verbal form from the items of the original list, does not approach sufficiently close to the original list to show the predicted inversion. That is, two words may be identical in meaning but vary widely in form, as words. This dual presence of form and meaning can hardly be escaped. It can scarcely have affected our results, as they stand, since as closeness of relation has been decreased form has varied, in the adjective lists, in no particular direction. It may, however, have prevented the appearance of an inversion. Should this be the case, it is doubtful if the Skaggs-Robinson prediction can be verified with meaningful materials, since the form of such materials can scarcely be held constant while the meaning is varied.

Conclusions

(*1*) The learning of interpolated lists of synonyms, antonyms, unrelated adjectives, nonsense syllables, and 3-place numbers produces retroactive inhibition in amounts which decrease steadily from

synonyms to numbers. This decrease is interpreted to be a function of the character of the materials.

(*2*) The learning of interpolated lists of adjectives grouped in three divisions with respect to their degree of synonymy to the original lists yields amounts of retroactive inhibition which vary directly as the degree of synonymy.

(*3*) These results are interpreted in favor of a transfer theory of retroactive inhibition. They do not, however, follow the Skaggs-Robinson prediction.

THE EFFECT OF INTERPOLATED WEIGHTS UPON COMPARATIVE JUDGMENTS

By J. P. Guilford and Dorothy G. Park, University of Nebraska

This study was inspired by a very fundamental theory of psychophysical judgments which is upheld by the Gestalt psychologists. Koffka, in discussing perception, makes the assertion that in the phenomenology of comparing and judging not two discrete objects are perceived, but always a step or gradation when one object is judged greater than another.[1] Koffka's statement was made with regard to pairs of grays presented simultaneously. It might be assumed that his arguments would be just as vigorously applied to the judgment of successive pairs of stimuli. The phenomenal step or gradation in the latter case would be temporal rather than spatial. But he does not seem to uphold this contention in its extreme form when discussing successive comparisons.[2] That is, he does not insist upon a 'conscious' gradation. He does insist upon the step-wise phenomenon, however, but in terms of the dynamic neurological processes which underlie the judgments.

The whole process of judging successive stimuli may be described in terms of *Gestalt theorie* as follows. The first stimulus of the pair, a lifted weight, for example, establishes a level of potential, or at least a *status quo* among those processes having to do with the lifted weight, and this new level tends to persist for a time. The second weight produces a change in this level, a shift of potential upward or downward. It is this shift that gives rise to the judgment of 'greater,' 'equal,' or 'less.'

The writers hoped to test Koffka's theory by inserting between the standard weight and its comparison stimulus a third stimulus which would tend to break up the simple gradation or step formed by the pair. Such an interruption should interfere to a great extent with the comparative judgments by breaking up the simple relationship between the pairs to be judged.

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[1] Kurt Koffka, Perception: an introduction to the *Gestalt-Theorie*, *Psychol. Bull.*, 19, 1922, 537 ff.

[2] K. Koffka, *The Unconscious*, 1927, 50 ff.

The choice of an interpolated stimulus was determined by several factors. It should not be simply a distractor. The uncertainty of the results to be expected from distractors is well-known by all who have worked with them. We merely wanted to destroy the direct continuity which existed between the two stimuli of a pair without disrupting too much the task of the observer. A third weight, one that was distinctly different from the weights of the pair, therefore seemed to be the best thing for this purpose. It would be a poor distractor because of its similarity to the other two stimuli and yet it should tend to operate directly upon the shift of potential if such a thing exists in this experiment.

Just what should an extra weight do to this purported change in potential? If it is heavier than the two stimuli that are being compared, it should raise the level left by the first weight and leave sufficient conditions for a step down when the third weight comes. A lighter interpolated weight should have the opposite effect. But the mental set of *O* would have something to do with the results. The *O*s were instructed to compare the first and third weights, judging whether the third was heavier or lighter than the first. This instruction was carried out to the extent that frequently the *O*s reported "The second weight tends to drop out; to be ignored." This might mean that the interpolated weight became less effective in altering the trace of the first. The objective data will show that this was true, but only to a slight extent. To what extent, on the whole, the mental set acted against the effect of the extra weight we are unable to say.

If comparative judgments depend upon a good, clear shift of potential, the interpolation of a third weight should have another effect. It should work against good discrimination and so enlarge the differential limen. It might also interfere with the comparisons in a way that would change the usual psychometric function. The data might not fit the phi-gamma curve so well. On the other hand, if the comparative judgments depend upon two distinct impressions and an act of comparing, *O* might conceivably rule out the middle stimulus successfully, and aside from any distracting effect his judgments would be just as accurate and would conform to the psychometric law as well as without the interpolated weight. The distracting effects, as we have said before, were reduced to a minimum

in these experiments, so that whatever effects we get may be regarded as due to the properties of the interpolated stimuli and their temporal relations with the other weights.

Method and Procedure

A series of 7 weights was used, ranging from 185 to 215 grm. with a 200 grm. standard. The interpolated weights were 100 grm. and 400 grm., thus chosen because the 200 grm. standard is the geometric mean of the two. It was assumed that they would have equal opposite effects upon the standard. To secure a normal (N) series for each *O*, with which to compare the A-series (400 grm. interpolated) and the B-series (100 grm. interpolated), a 200 grm. weight was also interpolated. It was thought that this would be a better normal series for two reasons. The interpolation of an extra weight in the A- and B-series increased the time interval between the standard and its comparison weight to 7 sec. The trace of the S-weight (the standard) probably changes rapidly during the first few seconds and so a 7-sec. interval had to be used also in the N-series. A blank pause of 7 sec. in the N-series would have altered conditions too much. The whole rhythm would have been different. The 200 grm. weight was therefore inserted in the N-series. Since the S-weight was always given first, the introduction of the same weight should merely intensify and prolong the trace set up by the first weight in the N-series. This should stabilize the hypothetical level against which the V-weights (variables) are to appear and so lead to better discrimination than usual. Only one *O* knew that the extra weight in the N-series was the same as the standard. All could not help knowing the relative weights of the other interpolated stimuli.

The procedure in the experiments was as follows. The weights were placed upon a revolving table so that each one could be brought directly under *O*'s hand. A bell-metronome was used to time the lifting of the weights. It was set to beat at the rate of a second with the bell sounding every fourth second. *O* was instructed to lift a weight every 4 sec.; 1 for lifting and 3 between the successive members of the group. A pause of 8 sec. was given between successive groups. 'Equal' judgments were not permitted. The standard was always given first. This leaves a time error in the data but it does not at all invalidate a comparison of the results from the A-, B-, and N-series. The three kinds of series were alternated according to a prearranged plan so that every kind followed every other equally often. Within every series, A, B, and N, the order of the V-weights was fixed according to a plan which permitted every V to follow every other equally often. This is important, for even with an interval of 8 sec. between a final V and the S of the next comparison, there is undoubtedly a persistent effect from one comparison to the next, as Fernberger has previously pointed out.[3]

In the N-series, every one of the V-stimuli was compared with the S-stimulus 180 times. In the A- and B-series only 5 of the V-stimuli were used, every one of which was compared with the S-stimulus 200 times. In the A-series (400 grm. interpolated), the V-stimuli were 185, 195, 200, 210, and 215. In the B-series

[3]S. W. Fernberger, Interdependence of judgments within the series for the method of constant stimuli, *J. Exper. Psychol.*, 3, 1920, 126-130.

(100 grm. interpolated), they were 185, 190, 200, 205, and 215. This limitation of the A- and B-series was primarily for the sake of saving time. It was thought preferable to secure a greater number of judgments upon 5 stimuli rather than fewer upon 7.

The observers were the senior author (*G*), Miss Ruth A. Hilton (*H*), graduate student who had some training in observation, and Miss Mamie J. Meredith (*M*), instructor in English who had had no training in psychological observation.[4]

Results

The results consisted of the proportion of judgments 'greater' for every V-stimulus in the A-,B-, and N-series. It was predicted that if the Gestalt theory were true, the interpolation of extra weights would have three effects. It would increase the limen and perhaps interfere with the usual psychometric function. The data would deviate significantly from the *phi-gamma* function. The third result expected was a shift in the number of 'greater' judgments. increasing them when the 100 grm. weight was interpolated and decreasing them when the 400 grm. was inserted. This shift can be expressed in terms of the point of subjective equality (Xi) which, according to expectations, should become smaller in the B-series and larger in the A-series. The limens, Xi, and the Pearson coefficients for goodness of fit were computed and are given in Table I.

TABLE I

Showing the Limens, the Points of Subjective Equality, Pearson Coefficients for Goodness of Fit, the Slopes and the Y-Intercepts for the N-, B-, and A-Series of Experiments

O	Limens			(Xi) Points of subjective equality			Pearson coefficients for goodness of fit			Slopes		y-intercepts	
	N	B	A	N	B	A	N	B	A	B	A	B	A
G	8.7	10.7	11.4	196.3	191.3	202.2	.00	.00	.69	.75	.62	.53	−.43
H	9.4	11.4	14.0	194.3	186.4	196.6	.00	.39	.96	.81	.70	.77	−.30
M	17.0	22.7	17.0	186.9	187.0	198.8	.87	.22	.82	.83	1.00	−.10	−.78

So far as the limens are concerned, the expectations were fully verified. With only one exception, the limens in the A- and B-series are larger than in the N-series. The limens in the A-series are larger than in the B-series for the two practiced *O*s but not for the third. The probable errors of these limens vary directly with the limens

[4]The data were collected by the junior author who acted as experimenter throughout the observations; the article was written by the senior author.

themselves and they range from 0.32 to 0.77. All the *O*s—even *M*, whose A-limen is not apparently disturbed—reported that the 400 grm. weight disturbed the judgments most of all.

The Xi are as predicted in almost every case. They range from 186.9 to 196.3 grm. in the N-series, which shows the extent of the time error. That is, a second weight of 196.3 grm. feels equal to the 200 grm. standard for *G*, and one of 186.9 grm. feels equal to the standard for *M*, just because it comes second. The Xi were lower for two *O*s in the B-series and equal to that of the N-series for the third *O*. They were significantly higher in the A-series than in the N-series for all *O*s. The probable errors of these values vary directly with the limens. They range from 0.81 to 2.03. Thus, the interpolated weights altered the impression of the 200 grm. V-weight, to the extent of 10.9 grm. for *G*, 10.2 grm. for *H*, and 11.8 grm. for *M*. These values were obtained by finding the difference between columns 5 and 6 of Table I, which is to say that a 200 grm. weight felt about 10 to 12 grm. heavier when following a 100 grm. weight than when following a 400 grm. weight at an interval of 3 sec.

Columns 7, 8, and 9 of Table I tell an unexpected story. The data seem to fit the *phi-gamma* function much better in the A- and B-series than in the N-series. This does not look so well for the configurational hypothesis, according to the above reasoning. Closer examination of the coefficients for goodness of fit[5] will show several absurdities and will make one skeptical of this method as applied to these data. It will be noticed that the less practiced the *O*, the better the fit! Another absurdity consists in the fact that the better the discrimination, or the lower the limen, the worse is the fit. In the latter statement lies the explanation of the former. When the limens are large, the proportions of 'greater' judgments have a rather narrow range, lying nearer 0.50 than they do to either extreme. When the limens are smaller, proportions approaching 0.00 and 1.00 occur. A discrepancy of an observed proportion from its corresponding theoretical proportion is weighted in finding the Pearson coefficient, and the farther from 0.50 the greater the weight. At a proportion of 0.50, a discrepancy is weighted by 4.0; at 0.90 it is weighted by 11.1, and at 0.99 the weight is 101.[6] Small wonder that the data from the same range of stimulus-weights give a poorer coefficient of fit when the limen is small! We would be justified in comparing the coefficients

[5] W. Brown and G. H. Thompson, *Essentials of Mental Measurement*, 1925, 79 f.
[6] *Ibid.*, Table 4, p. 204.

in Table I only if the limens are equal. Thus, for *M*, series A and N have equal limens and the coefficients are about equal and would be regarded as very good, except that we are led to be skeptical of the whole method. About the only comfort to be derived from these coefficients is that we are justified in using the constant process with the data from the A- and B-series.

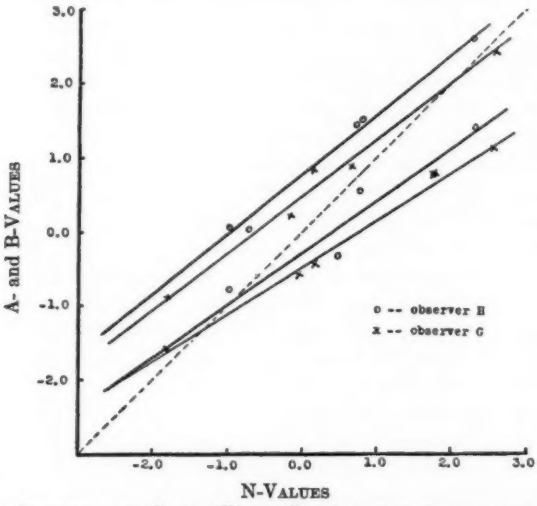

FIG. 1. SHOWING, FOR *G* AND *H*, THE RELATIONSHIP BETWEEN THE PSYCHOLOGICAL VALUES OF THE WEIGHTS AS FOUND IN THE A- AND B-SERIES IN RELATION TO THE CORRESPONDING VALUES IN THE N-SERIES

The dotted line is the function y = x which represents the relationship that would have obtained if the interpolated weights had had no influence upon the comparative judgments. The two upper lines are the B-functions and the two lower ones are the A-functions.

The writers wish to introduce at this point another method of treating the results which is new and which promises to be very fruitful not only in this problem but in many others. It consists in finding the psychological values for all the V-stimuli under the different conditions imposed upon them. The theory upon which the method is based has been discussed recently by Thurstone[7] and earlier by Fullerton and Cattell.[8] In order to find the psychological distance between any two stimuli, or rather the two psychological impressions of those two stimuli, upon a continuum, one need only know what proportion of the time the one is

[7]L. L. Thurstone, Psychophysical analysis, *Amer. J. Psychol.*, 38, 1927, 368-389.

[8]G. S. Fullerton and J. McK. Cattell, *Perception of Small Differences*, 1892.

judged 'greater than' the other. In these experiments the proportion of judgments 'greater' for every V-stimulus as compared with 200 grm. is known. By referring to Cattell's tables,[9] these proportions can be translated into linear scale values in which the unit of the scale separations is 1 p.e. We are then able to compare the psychological values of each weight in the A- and B-series with the corresponding values in the N-series. And we will assume that the psychological values of these weights measure their relative trace-values in the nervous system. Any significant difference between the same stimulus-weight in the N-series and in the A- or B-series may be regarded as due to the influence of the interpolation of 400 grm. or 100 grm. respectively.

The results, so treated, are most clearly seen in graphic form in Fig. 1. The values obtained in the A- and B-series (A-values and B-values) are plotted on the ordinate as functions of the corresponding values of the same stimuli as found in the N-series (N-values). The N-values were taken as the basis for comparison rather than the physical values of the weights or even the logarithms of the physical values, for various reasons. The physical values of the weights may not be the only objective factors influencing the comparative judgments. When physical values are used, even as in the constant method, we are assuming a linear relationship between these values and the psychological values. This assumption is not true because of Fechner's law, if for no other reason. In the method which is here proposed, the assumption need not even be made.

It has been seen by the other method, in which the constant process was used, that the 400 grm. weight caused the V-weights to lose in psychological value and that the 100 grm. weight had the opposite effect. The shift in Xi gives an idea of the degree of change in grams, but it does not tell us whether every V-weight was influenced in the same direction and by the same amount. The natural assumption, upon knowing only the amount of shift of Xi, would be to assume that every weight was so influenced. This was not true. For only one *O*, (*M*), was it so, and then only in the A-series. All 5 weights lost 0.78 p.e. units uniformly for *M*, when following the 400 grm. weight. In all other cases there is general agreement. In the A-series, the heavier the V-weight the more it lost. Below a certain weight, the stimulus actually gained instead of lost! In the B-series, conversely, the lighter the V-weight the more it gained. Above a certain value the stimuli actually lost instead of gained. The functions (see Fig. 1) are all linear and it will be observed that the best-fitting lines, adjusted by the method of least squares, touch most of the points, indicating that the laws which have just been stated would apply beyond the range of V-weights which were used.

This is a surprising result: that both the interpolated weights should make some of the V-weights heavier and some lighter, but in a regular and lawfnl fashion. Further experiments need to be done beyond the range which was used to see whether the linear functions continue indefinitely, or whether the A- and B-functions become asymptotic to the line $y = x$.

The slopes of the linear functions are in close agreement for two *O*s (*H* and *G*) as will be seen in Table I, columns 10 and 11. That is, the slopes of the A-functions are less than 1.00. Now it is quite probable that these smaller slopes are

[9] *Ibid.* p. 16.

due to the poorer discrimination in series A and B. The limens which are given in Table I show what might be expected. That is, the ratios of the A- and B-limens to the N-limens are inversely proportional to the slopes of the A- and B-functions also given in the table.

Corresponding to the values of Xi as obtained by the constant process, in this method we have the y-intercepts of the linear functions. These values are given in Table I, columns 12 and 13. They are all in agreement, that is, negative in the A-series and positive in the B-series, except for the −0.10 for the unpracticed *O*. These results are transposable into the terms of the old method and hence agree with what has already been said.

The results so far considered must not be regarded as merely the direct effect of the interpolated weights upon those immediately following. There is evidence, mostly of an introspective nature, to show that there is a strong dynamic interrelation between every weight and every other one lifted during a single period of obervations. Two *O*s reported an apparent shift in the standard weight itself. It is to be remembered that the interpolated weight was lifted 20 times in each A- and B-series, and that only 11 seconds elapsed between the lifting of the second weight and the next following standard. We do not know just how long the trace of any weight remains effective, but the fading probably follows a function similar to Ebbinghaus' curve of retention. Certainly a weight that is strikingly different from the others of a group and that is given repeatedly must have a cumulative effect upon all the others. At any rate, the *O*s reported that the standard underwent a progressive change. In the A-series it became gradually lighter; in the B-series progressively heavier. There is some objective check upon this subjective impression. In the A-series, if the impression of the *O*s is true, the number of 'greater' judgments should increase as the series progressed. Since each A-series consisted of 20 comparisons, four with each V-weight, we can fractionate the data into four parts and note whether there were more 'greater' judgments as we go from Part I to Part IV. There was no uniform progression in the number of 'greater' judgments for any V-weight, and there was but slight indication of this if we combine the 'greater' judgments for all 5 weights in Parts I to IV. For *G* these totals are 100, 93, 100, and 117, in Parts I, II, III, and IV respectively. For H they are 115, 113, 109, and 123. Likewise, in the B-series, one might expect a decrease in the number of 'greater' judgments from Part I to Part IV. Again, there is but little indication of this, For G the totals of 'greater' judgments were 136, 119, 126, and 133. For H they were 161, 135, 127, and 141. If one con-

siders only Parts I and IV of the series of 20 observations, the expected change does occur in both the A- and B-series.

In order to be more sure of this, we have computed Xi for Parts I to IV in both the A- and B-series. They are given in Table II, columns 3 and 4. The results are so variable that no certain conclusions can be drawn. There is but a feeble tendency for Xi to gravitate to a lower level in the A-series and to rise to a higher level in the B-series, but it is more or less true for all the *O*s.

TABLE II

SHOWING THE LIMENS AND POINTS OF SUBJECTIVE EQUALITY FOR EVERY ONE OF THE FOUR PARTS OF THE EXPERIMENTS

O	Part	Limens		Xi Points of Subjective Equality	
		A	B	A	B
G	I	11.4	15.4	202.8	185.8
	II	13.3	9.0	204.9	193.8
	III	10.2	9.2	202.8	192.2
	IV	9.5	10.4	199.2	190.3
H	I	21.7	11.9	195.7	178.2
	II	10.7	9.6	198.6	188.9
	III	17.6	9.6	198.5	191.0
	IV	11.1	13.3	196.2	184.3
M	I	20.2	28.1	202.9	178.0
	II	14.7	20.2	195.2	188.0
	III	15.4	21.5	202.1	198.0
	IV	22.7	23.2	197.5	185.7

There is, of course, a good reason why the objective results do not bear out the introspective reports on this point any better than they do. For while the S-weight is phenomenally changing, the V-weights are also making a corresponding shift, less noticeable to *O* perhaps, because he observes that the V-weights differ from each other. This coincident change in the V-weights is to be detected only indirectly in some of the introspective reports. In such a typical report, *O* would say that the S-weight had become so light (in the A-series) that he was sure of a 'greater' judgment the moment the first weight came. But when the V-weight came, it was so unexpectedly light that the judgment had to be reversed to 'less.' Very likely the conditions of the experiment have not yet been so arranged as to give a true objective picture of the many dynamic interplays that are going on. It will take many different variations of the experiment to bring them all out, but the writers believe that a beginning has been made in this direction.

Another report of all the *O*s is worthy of objective check. It is the report that as each series progressed the middle weight seemed to

drop out or to interfere less and less with the comparison. This is an important item for the solution of our original problem. This should mean better discrimination as the series was run through, and so a smaller limen. The fractionated data were used again to test this subjective impression. The limens were computed in Parts I to IV of the data in series A and B for all *O*s, and are given in Table II, columns 1 and 2. The trend to be expected in the limens is quite clear for *G*, but less so for the other *O*s. All *O*s show another distinct tendency, not even hinted at in the introspections, and that is to have maximal discrimination in Parts II and III, but a lapse in Part IV that gives almost as large a limen, on the average, as in Part I. There seems to be nothing in the data to explain this fact, if it is not due to chance.

But what of our original problem, to determine whether or not the comparative judgment issues from a shift in the status of the impression, conscious or unconscious, of the first stimulus when the second one arrives? The argument has been that if there is such a shift or step in comparative judgments, an interpolated weight should interfere with it in certain ways. For the most part the test gave positive results. The issues are not as clear as one might wish under the conditions which were set up. The additional weight did not serve simply as a distractor for there was a third weight present in the N-series as well as in the A- and B-series. Phenomenally, the extra weight had an effect that is in agreement with the 'step' theory. As the *O*s repeatedly expressed it, "The first weight seems to have a definite value, relatively heavy or light, but the second weight seems to 'take the edge off of it.' " If one is willing to admit that brain processes can be described by introspection, what better description could one want for a shift of potential or any status of some kind, than this? Furthermore, the objective results fit nicely into the Gestalt theory and the configurational terminology seems to describe them naturally. There seems no other way of viewing the changes produced by the interpolated weights than to regard them as a series of dynamic interrelations which must be studied by a systematic variation of experimental conditions such as have only been suggested in this article.

Summary

A heavier and a lighter weight were interpolated between a standard and its comparison weights with two distinct results: (a)

they enlarged the differential limen, making discrimination poorer; (b) they shifted the psychological values of the variable weights relative to the standard. A heavy interpolated weight tended to decrease the impressions of the variable weights and a light interpolated weight tended to increase them. But all variable weights were not affected to the same degree nor in the same direction by the same interpolated weight. A method was introduced by which the dynamic interrelation of lifted weights may be studied under different variations of conditions. Evidence both objective and subjective favors the Gestalt theory of the comparative judgment.

THE PRESSURE CORRELATE OF EMOTION

By William A. Hunt, Harvard University

This report deals with an investigation of the pressure components of emotional experience.[1] In a previous series of experiments the writer confirmed the correlation shown by Nafe between bright pressure and pleasantness and between dull pressure and unpleasantness.[2] It then seemed promising to extend the investigation to cover the felt emotions, as the results of such work promised to be interesting from any one of three angles. First, the presence or absence of bright pressure in the pleasant emotions and dull pressure in the unpleasant emotions would offer an important check upon the previously established correlation. Next, the examination of the pressures experienced in these stronger, emotional situations would reveal further characteristics of the pressure experience, and would offer further opportunity for comparing it with the affective experience. Finally, it was possible that the investigation would yield a new method of approach for the study of the emotions.

Method and Procedure

Although the experiment as a whole was divided into four parts, the general technique was the same for all of them. The *Os* were given the following descriptions of bright and dull pressure:

'Bright pressure' resembles tactual bright pressure, or a touch quality lying between bright pressure and tickle. It is a diffuse bodily feeling with expansive qualities. In addition to its decided brightness, it may be described as buoyant, lively, elusive, and welling. It is like expansiveness of the body.

'Dull pressure' has been described as dull pressure or a quality lying near dull pressure, or between dull pressure and strain. It is not extended, and has qualities of constriction. It is dull, inert, heavy, sinking, leaden, condensed, and like bodily contraction.

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[1]The author extends to Dr. Beebe-Center of the Harvard Psychological Laboratory his appreciation of the assistance and encouragement offered him.

[2]W. A. Hunt, The relation of bright and dull pressure to affectivity, this Journal, 43, 1931, 87-92; cf. also J. P. Nafe, *ibid.*, 35, 1924, 507-544.

The *O*s were then subjected to various emotional situations, and were asked to note the presence or absence of bright or dull pressure in their experience, and to give a brief introspection of their feelings during the experience. Elaborate reports were welcomed, but if these were not forthcoming no attempt was made to force their occurrence. The emotional situations used as stimuli were of various sorts and differentiated the experiment into the four parts.

Experiment 1. In this first part the *O*s used were a group of 9 graduate students in psychology, all members of the Harvard Psychological Laboratory. The introspections were obtained immediately after they had been informed of their results in the language examinations preliminary to the Ph.D. degree. As the results of these examinations were very important to the men involved, it was assumed that knowledge of success would entail a pleasant emotion, such as relief or elation, and knowledge of failure an unpleasant emotion, such as disappointment. The *O*s were taken singly into a room and seated in an easy chair with a writing board across the arms. They were then handed the description of bright and dull pressure and asked to peruse it carefully. The description was then removed, and *E* gave the *O* a sealed envelope containing the result of his examination. Thus *E* also was ignorant of the result and could not furnish any clues to *O*. Upon offering the envelope to *O*, *E* said: "I am going to give you your result and ask you to identify the presence or absence of bright or dull pressure. Give a frank introspective report of your feelings." *E* then retired and left *O* to his report.

Experiment 2. As it was found in the situation above that the expectedness of the result often robbed the situation of any emotional content, the following procedure was devised for another group of 7 *O*s. It was designed to study the emotional experience aroused when *O* expected to receive a shock, and his relief when he learned that he was not to receive one.

The *O* was given the description of bright and dull pressure and asked to read it carefully. He was then seated in an easy chair with his feet on a cork 'insulating' mat. The room was darkened. On a table before *O* was a large enamelled rheostat, 16 in. in diam. Above the rheostat, on a separate circuit, was a Ford spark coil designed to give a half-inch jump-spark through wires at one end. On the rheostat circuit at the lower right hand corner of the rheostat was an electric projection bulb giving an intense white light. On the wall at the left was a red light. Hidden on the back of the chair behind *O*'s head was an electric buzzer. *E* put on a pair of smoked glasses and stepped behind a partition in back of the table. He turned on the red light and asked: "Is the red light on?" When *O* said "Yes," *E* came around in front of the table and threw on the spark in the Ford coil. He then stepped back behind the partition and connected the rheostat circuit causing the projection bulb to light. Then he came forward again and gave *O* a pair of brass electrodes to hold. These were connected with a switch at the base of the rheostat. *E* then commenced to 'step up' the current in the rheostat, causing the projection bulb to give off an increasingly bright white

light. At the same time the buzzer behind *O*'s head was connected. When the rheostat was turned on full, *E* turned to *O* and asked for an introspection in this preliminary situation. The current was shut off while *O* wrote. When he was through, he was told: "I am not going to shock you. It would be impossible with this apparatus as the electrodes are not connected with any current." Then *E* again asked for an introspection in this new situation.

Experiment 3. This experiment was devised to add data from further emotional situations that might be called revulsion, amusement, and fright. *O* was seated in an easy chair with a writing board before him. He was shown the description of bright and dull pressure, and was told: "I am going to ask you to identify the presence or absence of bright or dull pressure in some experimental situations." A covered box was then brought in and placed on a chair beside him. There was a hole in one end of the box through which the *O*'s hand could enter. Within the box and hidden from sight was a pan containing an over-ripe banana broken in two and covered with machine oil. *O* was told: "Put your hand into the box and feel of the contents for about 10 sec. Then give me a brief introspective report of your feelings, mentioning particularly the presence or absence of bright or dull pressure." This was Situation 1, and was intended to arouse an unpleasant emotion. After *O* had finished writing, *E* approached and asked: "Would you like to see what you have been handling?" Then the banana was exhibited, and *O* was asked for another report. This was Situation 2 and was intended to arouse amusement. After this report, *O* was asked to concentrate carefully on the differences between the two preceding experiences. While he was thus engaged, *E* fired a revolver behind him. He was then told: "Let me have a brief introspection in this final situation." The word *final* was inserted to relieve him of any fear of further shock and to enable him to concentrate on his report. This was Situation 3 and was designed to cause fright. Six *O*s from the previous experiment were used.

Experiment 4. This duplicated exactly the routine of Experiment 3, but a group of 6 wholly naïve *O*s were used. None of them had ever done any previous psychological observing, and all of them were entirely ignorant of the problem involved. Thus they served as checks upon the sophisticated *O*s used in the first parts.

Results

In view of the fact that the results of every experiment are of the same general nature, involving the recognition of bright or dull pressure during an emotional experience, it seems best to treat them together. In all, 59 introspective reports were collected. Sixteen of these were discarded because they contained no mention of either emotion or pressure. In these cases the experimental situations proved ineffective as stimuli for either of the experiences under investigation. There were three other cases that were useless because of the failure of the *O*s to carry out the instructions to concentrate on bright and dull pressure. In these three, one man was so frightened

by the shot that he was unable to introspect, one man criticized the instructions as being vague and unscientific and claimed he could make no intelligent judgments for that reason, and one man reported verbal associations due to his failure to maintain a sensory attitude in this introspection. The 40 remaining cases may be tabulated as follows:

Number of cases	*Emotion*	*Pressures reported*
2	disappointment	dull
1	suspense	dull
10	fear	dull
1	fear	none
6	revulsion	dull
1	revulsion	dull (doubtful)

The above cases may be classified as unpleasant emotions. The pleasant emotions follow:

1	expectation	bright
3	elation	bright
8	relief	bright
1	relief	bright (doubtful)
5	amusement	bright
1	amusement	bright (doubtful)

In the table above 'doubtful' indicates a positive identification, but one lacking absolute certainty on the part of *O*.

Thus we have 19 definite cases of dull pressure in unpleasant emotions, and 17 cases of bright pressure in pleasant emotions. In addition we have one report of doubtful dull pressure in an unpleasant emotion, and two reports of doubtful bright pressure in pleasant emotions. In a single case there was reported an unpleasant emotion with no apparent pressure correlates. In no case was bright pressure reported with an unpleasant emotion, or dull pressure with a pleasant one.

The incidental material reported by some of the *O*s was very interesting. Three of the *O*s who had observed in the previous experiment upon the pressure correlates of affectivity reported that the pressures were qualitatively the same as those accompanying mere pleasantness and unpleasantness, with the bright pressures about equal in intensity to those in the affective situations, but with the dull pressures grading from equality to a much greater intensity than that experienced in the purely affective reaction. This suggests interest-

ing implications for any theory of emotion, but is not appropriate to develop them here. In general, the dull pressures seemed more intense and more definitely localized than the bright pressures. There were 13 cases of spontaneous localization, bright pressure being localized in the chest, and dull pressure in the abdominal or stomachic region. This localization was wholly spontaneous as no instruction was given for it by *E*.

Four *Os* reported something like an instantaneous bright pressure following the revolver shot. It was fairly intense, but was so rapid and fleeting that introspection was almost impossible and a definite identification could not be made. It came immediately after the shot and had no affective or emotional concomitants. It was followed at once by the usual dull pressure accompanied by fear and unpleasantness. This paradoxical bright pressure presents a problem, but perhaps a clue may be found in the work of H. Strauss.[3] Strauss took moving pictures of the reaction to a revolver shot for a number of *Os*. He found a primary reaction immediately after the shot which involved a general contraction of the body. This contraction came and went within the space of half a second, and was followed by a secondary reaction presenting the usual manifestations of fear. This result suggests a possible relation between bright pressure and a release of energy which the writer has followed in other manifestations but which he is not ready to discuss at this time.

Conclusions

On the basis of these results, it is possible to draw positive conclusions in each of the three directions mentioned in the introduction.

In the first place, these results offer further confirmation of the previously established correlation between bright pressure and pleasantness and between dull pressure and unpleasantness, and we may conclude with increased confidence that pleasantness is definitely related to bright pressure and unpleasantness to dull pressure.

Secondly, the intensive features revealed by these pressures in emotional experience follow closely the characteristics of the affective components of emotion; namely, that the pleasantness of an emotion is no more intense than that of a purely affective situation, while the unpleasantness experienced in an emotion may be much more intense than that experienced in the affective reaction.

[3]H. Strauss, *J. Psychol. u. Neurol.*, 39, 1929, 111-232.

The previously reported localization of bright pressure in the chest and dull pressure below it in the abdominal region receives further support from this work.

A 'paradoxical' bright pressure was revealed.

Finally, it appears that study of the pressure components of emotion is valuable, not only because of its revelations as to the the nature of the pressure experience, but also because of its implications in treating the emotional experience itself. The modified method of correlation used here may well be given broader application in the study of the emotions, and need not be limited to the study of the pressure components of the experience.

THE MEASUREMENT OF IMPLICIT SPEECH REACTIONS

By G. H. Rounds and A. T. Poffenberger, Columbia University

Implicit speech reactions in so-called 'inner speech' have been measured in a variety of ways. They have been studied directly (1) by attaching a tambour to the larynx;[1] (2) by inserting a tambour of some sort into the mouth or attaching it to the tongue so that tongue movements will affect it;[2] (3) by fastening a vacuum cup to the tongue and recording its movements in the horizonal and vertical planes by means of mechanical levers;[3] and indirectly (4) by looking for disturbances or inhibitions of mental processes when the vocalizing mechanisms were otherwise engaged—as by reciting the syllables "la, la, la," or numerals, or singing a note of predetermined pitch, or singing "ah."[4]

The results of these studies with their varying technique have been conflicting. The direct methods have shown both the presence and the absence of implicit movements in inner speech resembling those in overt speech; while the indirect methods have shown both that 'interfering' speech reactions disturb thinking and that they create a facilitating effect of no small amount. In all cases the technique, whether of the direct or the indirect sort, introduces an obstacle to the normal or customary functioning of the speech mechanisms. Since the study of Thorsen has shown that the extent of tongue movements is more directly related to nervous irradiation and muscular tonus than to any other factors that she studied, such obstacles constitute a real disturbing influence in the measurement of implicit speech reactions.

* Accepted for publication December 6, 1930.

[1] H. S. Curtis, Automatic movements of the larynx, this Journal, 11, 1899, 237-240.

[2] H. B. Reed, The existence and function of inner speech in thought processes, *J. Exper. Psychol.*, 1, 1916, 365-392; H. C. Courten, Involuntary movements of the tongue, *Yale Psychol. Studies*, 10, 1902, 93-96; A. Wyczoikowski, Theoretical and experimental studies in the mechanism of speech, *Psychol. Rev.*, 20, 1913, 448-458; R. S. Clark, An experimental study of silent thinking, *Arch. Psychol.*, 4, 1922, (no. 48).

[3] A. M. Thorson, The relation of tongue movements to internal speech, *J. Exper. Psychol.*, 8, 1925, 1-31.

[4] R. Fintner, Inner speech during silent reading, *Psychol. Rev.*, 20, 1913, 129-153; A. M. Thorsen, The relation of tongue movements to internal speech, *J. Exper. Psychol.*, 8, 1925, 1-32.

The work here to be reported is a demonstration of a method that is free from such obstacles and that has certain other advantages.

In connection with an investigation of the effects of work upon metabolism it was necessary to get a good breathing curve. The metabolism measurements were made by means of the Haldane-Henderson-Bailey method, using the Bailey face mask and valves, and the Douglas bags for collecting the expired air.[5] After an *S* has become adapted to the mask his breathing and articulation are not in the least interfered with and bodily movements are not inhibited. A person properly adjusted in the device can sit at a table and work in a normal fashion for hours, if necessary. Two tubes with an internal diameter of one inch lead off from the face mask. The one provided with an inlet valve brings fresh air to *S*; the other provided with an outlet valve carries the expired air to the Douglas bag. A side tube of slightly smaller diameter (7/8 in.) is led off from this latter tube between the mask and the valve, and joins a large tambour with a free rubber membrane 5 in. in diam. The proper balance between the resistance which the air column meets within the Douglas bag and that offered by the tambour is regulated by an adjustable clamp on the tube leading to the tambour. This tube has also to be restricted in order to keep the response of the tambour within a convenient range.

There is, then, a continuous column of air rising from the lungs, through the trachea, larynx, mouth, nose, mask, tube, and tambour chamber, meeting no more noticeable obstruction in its movement than exists under ordinary conditions of respiration. This column of air will be affected by the movements of the chest, by its passage through the larnyx, by the changing tension of the vocal chords, by the movements of the tongue, by the changing shape of the mouth cavity, and by its passage through the teeth and lips. Implicit speech reactions may involve any or all of these mechanisms. Whatever happens should show itself in the behavior of the column of air. The composite effects of these various factors can be graphically recorded by way of the sensitive recording tambour. Thus, whatever difference there is between rest and the presumed incipient speech, or between silent speech and speaking aloud should become evident from the appropriate comparisons.

The earlier work of Morgan showed that articulation accompanying mental work under distraction manifested itself in the breathing curves of his subjects, even when the records were made with the ordinary pneumograph.[6] "Some subjects as soon as they began work," he says, "articulated noticeably and continued to do so through the entire experiment, emphasizing the movements during the disturbances. Some did not do so noticeably until the disturbances came. The positive evidence that the more radical changes in the breathing ratio corresponded to the more extensive articulation and the weaker ones to lesser movements of the lips points to a relation between still smaller changes in ratio and weaker involuntary movements of the speech organs." Morgan's measurements were not fine enough to show more than this crude relationship

[5] P. B. Hawk and O. Bergeim, *Practical Physiological Chemistry*, 1926, 9th ed., Chap. 21.

[6] J. J. B. Morgan, The overcoming of distraction and other resistances, *Arch. Psychol.*, 3, 1916, (no. 35), Chap. 4.

between mental work and breathing, but did suggest that articulation could be caught in the breathing record.

The sensitive tambour actuated by changes in the air column as described above makes possible the recording of very fine movements of the vocal apparatus. A few records taken by this method will be presented for purposes of illustration. On these records a rising line means expiration and a descending means inspiration.

Results

The results of the study are shown in the accompanying illustrations (Figs. 1-6). Fig. 1 shows three curves: Curve 1 was taken while *S* was at rest, sitting comfortably before a table; Curve 2, while *S* was silently adding columns of 3-place numbers; and Curve 3, reading from right to left, shows the same work with a change to rest in the last third of the period. Fig. 2 likewise contains three curves: No. 1 was taken while the subject was at rest; No. 2, reading from right to left, shows the transition from rest to work; and No. 3, also reading from right to left, indicates a change from work to rest. Fig. 3 shows a more erratic *S*; first, the resting curve in No. 1, followed by work in Nos. 2 and 3.

Fig. 4 shows a comparison of two breathing curves. No. 1 was obtained while the subject was adding columns of 3-place numbers aloud, and No. 2 while he was adding silently. One can see more than a superficial resemblance between these two curves. A faster drum speed would make still more detailed comparisons feasible. The rapid fluctuations in the air column taking place at the end of expiration suggest some form of articulation. In Fig. 4 there is an additional irregular curve appearing in each case just below the breathing curve. This is a curve showing general bodily activity during the work. It is made by a method which will be described in a forthcoming article. In general, this *S* did one problem at the end of each expiration, finishing it before inspiring. It will be noted that he tends to sit fairly still while adding, but tends to shift or move at the end of each problem. A somewhat different picture is obtained in Fig. 5 for another *S*. No. 1 is a resting curve, No. 2 is a curve obtained during silent adding, and No. 3 during adding aloud. In this case there is much less evidence of speech reactions during silent adding than in the case of Fig 4, although even here there is a difference between the resting curve and the silent work curve.

As a further check on the method, records were made of the breathing of a person who was deaf and dumb from birth. These are

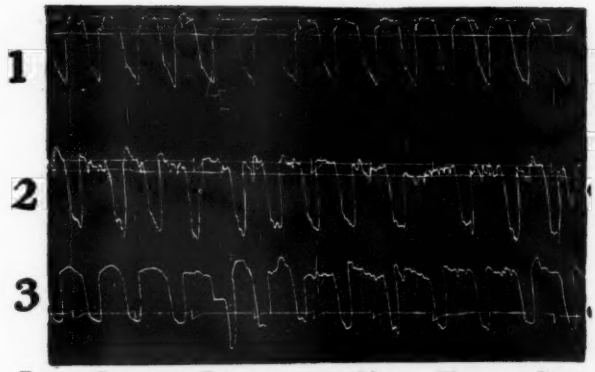


FIG. 1. BREATHING RECORDS DURING MENTAL WORK AND REST
(1) Rest; (2) Work; (3) Transition from work to rest

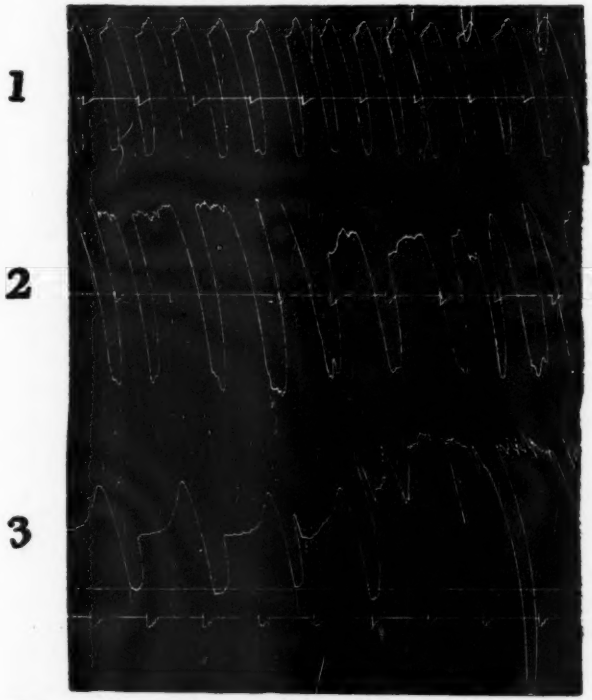


FIG. 2. BREATHING RECORDS DURING MENTAL WORK AND REST
(1) Rest; (2) Transition from rest to work; (3) Transition from work to rest

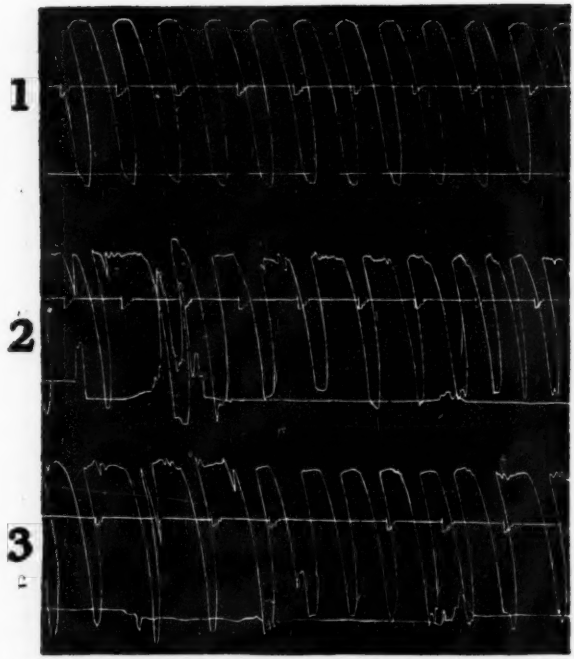


FIG. 3. BREATHING RECORDS DURING MENTAL WORK AND REST
(1) Rest; (2) and (3) Work

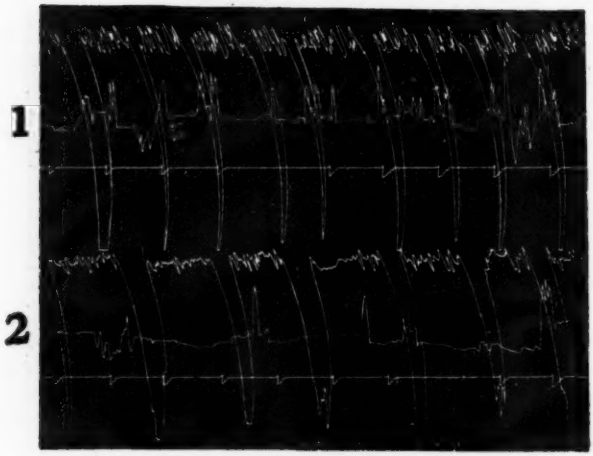


FIG. 4. BREATHING RECORDS DURING ADDITION
(1) Adding aloud; (2) Adding silently

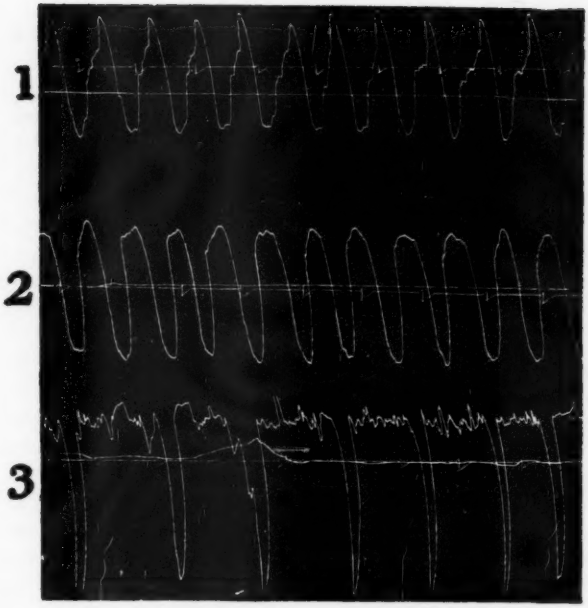


FIG. 5. BREATHING RECORDS DURING REST AND WORK
(1) Rest; (2) Adding silently; (3) Adding aloud.

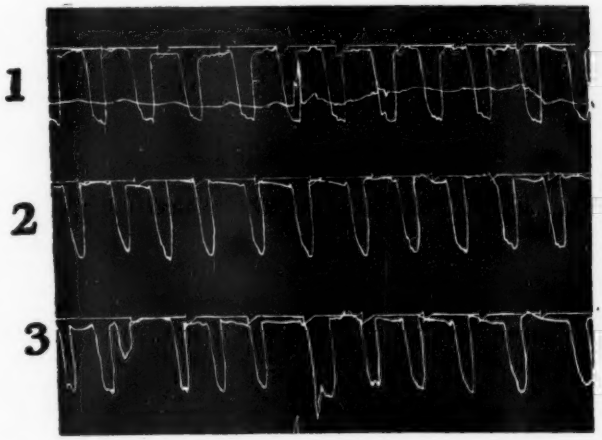


FIG. 6. BREATHING RECORDS OF A DEAF AND DUMB SUBJECT
(1) Rest; (2) and (3) Adding 3-place numbers.

shown in Fig. 6. Such a person, since he does not speak, should present a different picture from the normal person. Curve 1 is a resting curve. The slight breaks at the end of expiration are due probably to nervousness as this *S* was not adapted to the mask and other aspects of the experimental situation in the way that the other *S*s were. Curves 2 and 3 are records during the addition of three place numbers. They bear a very close resemblance to the resting curves. The periods at the end of expiration during adding are no more irregular than those at the end of expiration during rest.

These curves are merely samples of records that have been taken. Since they have been presented as a demonstration of a technique, there is no attempt at measurement or statistical treatment. They do, however, suggest a number of problems. Perhaps most interesting is the question of individual differences in articulation, or implicit speech, and the relation of such differences to proficiency of work and other measurable characteristics. Other problems, such as the nature of the breathing curves during different kinds of work and in rest, the nature of the breathing during transition from rest to work or from work to rest, the changes in breathing during prolonged periods of work, the changes in breathing during adaptation or adjustment to a given kind of work, the effects of distractions, the effects of varying difficulty of work, and many others were approached by these methods outlined here. Some of them are at present under investigation.

THE INFLUENCE OF COMPLEXITY ON THE FLUCTUATIONS OF THE ILLUSIONS OF REVERSIBLE PERSPECTIVE

By Wilma T. Donahue and C. H. Griffitts, University of Michigan

During the last part of the past century and up to about 1910 there was considerable interest in the illusions of reversible perspective. The first explanations of the phenomena offered were based on eye movements. It was thought that the part of the figure fixated was interpreted as being nearer the observer and that the changes in perspective were due to the shifts in the fixation-point. Why the fixation-point should shift rhythmically was not explained, consequently the theory was rejected by a considerable number of investigators in favor of a central theory. No one denied that the fixation-point ordinarily shifted, but those who posited a central theory denied that the shifts were the real cause of the changes in perspective.

Between 1900 and 1910 several writers suggested that the reversals in perspective were related in some way to the refractory phases of the neural processes involved, while others suggested that Sherrington's work with antagonistic and alternating reflexes furnished the basis for an explanation.[1] Since then these figures have received but little attention—not because anything had been settled concerning them, but because psychologists became interested in other problems.

It is unfortunate that the work with these figures was not carried to a conclusion because an adequate explanation of the phenomena involved will, as we believe, add much to our knowledge concerning the action of the nervous system. It was in the hope of contributing something to the understanding of these illusions of reversible perspective that we began a series of experiments, the first of which was to carry further the work begun by Gordon on the influence of what she called 'complexity' on the fluctuation rate.[2] She found that the book figure (our Fig. 6) changed more rapidly than the stairs figure (similar to our Fig. 1) and from this result concluded that complexity as such tends to make the rate slower. Although her conclusion has been generally accepted, the objective and introspective results of some preliminary work led us to doubt the validity of her conclusion.

It may be noticed that the main objective difference between her 'simple' figure and her 'complex' figure is in the number of parts or units. In the first figure there is only one object, consisting of two parts, but in the second figure there is a large number of parts. There are many more lines in the second figure. We therefore selected several simple and several complex figures, the two groups differing from each other in essentially the same way as her two figures differ.

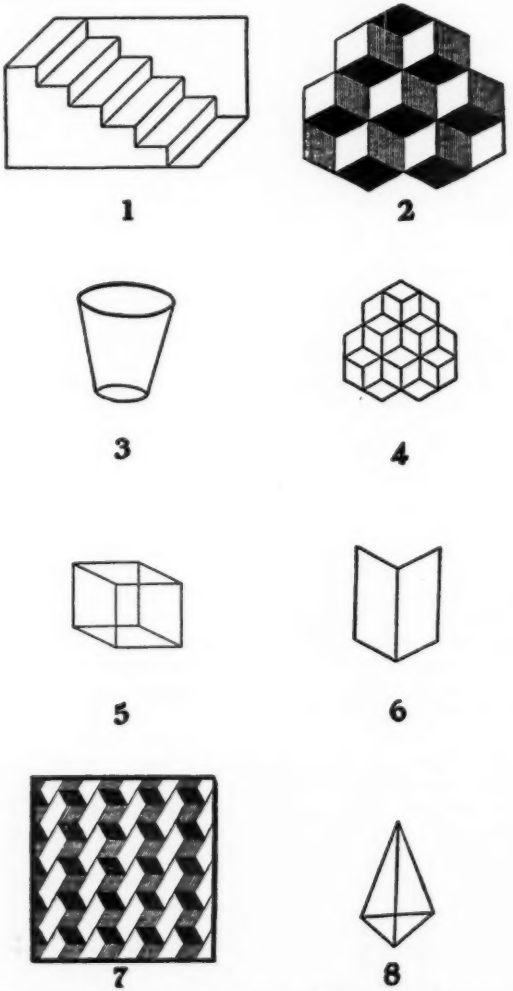
*Accepted for publication September 29, 1930.

[1] C. S. Sherrington, *The Integrative Action of the Nervous System*, 1906.

[2] Kate Gordon, Meaning in memory and attention, *Psychol. Rev.*, 10, 1903, 267-283.

Method and Procedure

The figures used in this study are shown in the accompanying drawings (cf. Figs. 1-8). They were selected from a number used in a preliminary series as being the most satisfactory for our purposes. The discarded figures too frequently lost all perspective.



Figs. 1-8. Showing Figures of Reversible Perspective Used in the Study

Photographs, 3 x 4 in. in size, were made of the selected figures, and the photographs were then pasted on 10 x 11 in. white cards. These cards were placed from 18-24 in. in front of *S*, at right angles to his line of regard. The cards were illuminated by a 100-w. electric lamp which was placed about 2 ft. above and behind *S*'s head. A polished reflector behind this lamp increased the intensity of the illumination of the figures. The walls and everything within *S*'s room were painted a dull black.

S reacted to the changes in perspective by moving a reaction-lever to the right for one phase and to the left for the other. With this apparatus it is not necessary for *S* to maintain a muscular tension during one phase as it is with an ordinary reaction-key. This precaution is necessary because it secures comparable muscular conditions during the two phases. The recording apparatus was in an adjoining room.

Six *S*s, A, B, C, D, E, and F were used. Two 1-min. records were secured from every *S* with each of the figures at every sitting. For any one *S* there was but one sitting a day, and ordinarily successive work-days were one week apart. At each sitting there was a rest period of about 2 min. after the taking of each record. There were 4 sittings for the first 3 *S*s and 5 for the last 3.

TABLE I

SHOWING FOR EVERY *S* THE MEAN FLUCTUATION TIME FOR EVERY FIGURE
(Time expressed in seconds)

	Figure No.	Subject						
		A	B	C	D	E	F	Mean
Simple	3	7.63	7.80	5.51	4.20	5.78	4.11	5.84
	5	8.87	6.39	4.39	4.42	2.53	6.00	5.43
	6	7.06	7.04	3.69	2.50	3.14*	2.91	4.39
	8	11.05	10.73	4.92	2.51	3.30	7.73	6.71
	Mean	8.65	7.99	4.63	3.41	3.69	5.19	5.59
	P.E.$_M$	0.52	0.56	0.23	0.31	0.42	0.62	0.34
Complex	1	8.28	6.49	3.81	5.00	2.57	7.62	5.63
	2	6.70	6.69	4.79	4.15	3.25	2.50	4.68
	4	5.28	6.44	3.95	3.44	3.09	2.78	4.16
	7	10.63	8.30	3.73	4.17	2.84	12.08	6.96
	Mean	7.72	6.98	4.07	4.19	2.94	6.25	5.36
	P.E.$_M$	0.67	0.26	0.14	0.19	0.09	1.33	0.35
	Diff.	0.93	1.01	0.56	−0.78	0.75	−1.06	0.23
	P.E.$_{diff}$	0.85	0.62	0.27	0.36	0.43	1.47	0.48

*In the course of our experiment, Fig. 6 soon ceased to fluctuate for E. Using the scanty data we secured, and by an interpolation to take into account the fact discussed in the last paragraph preceding the summary of this report, we found the value (3.14) which appears in the table. Its elimination will not affect any conclusion that might be drawn from the table. Without it the mean fluctuation time for all simple figures is 5.70 instead of 5.59. We do not know the cause of the difficulty this *S* had with this figure.

Results

(*1*) *The influence of complexity.* The mean cycle-lengths, in sec., for every *S* and every figure are shown in Table I. The complex figures fluctuated more rapidly than the simple figures for 4 of the 6 *S*s. These differences are, however, too small to be very reliable, although the chances are more than 2 to 1 that there is a genuine difference greater than 0 and in the same direction. If the cards are arranged for every *S*, in order according to the rate of change, we find that the average ranks of the complex figures is higher (faster rate) for 5 of the 6 *S*s. In this situation it is probable that this method of comparison is somewhat safer than the method of comparing group means.

Although our data do not furnish a final answer to the question of the influence of complexity on the rate of change, they do show quite definitely that we can no longer accept Gordon's general conclusion that complexity results in a slower fluctuation rate. With regard to the two figures she used, our data agree with hers. But it is evident that we do not dare to generalize on the basis of the data secured from this one pair of figures.

(*2*) *The influence of other factors.* A detailed study of our objective and introspective data revealed evidence of two general principles which we now wish to present.

(*a*) *Definiteness of ideas.* Other things being equal, the rate of change is proportional to the definiteness of the ideas involved in the two interpretations or appearances of a figure. If there is a high degree of definiteness in the ideas involved in the two interpretations of one figure and a low degree of definiteness in the ideas involved in the two interpretations of another figure, we believe that, other things being equal, the fluctuation rate will be faster in the first figure.

(*b*) *Balance.* Other things being equal, the rate of change is proportional to the approach to equality of certain aspects of the neurological activities involved in the alternative interpretations of a figure. This equality is in something that goes with the ability of an *S* to describe in detail the appearance of a figure in each of its phases, or in the ability of an *S* to image clearly the two appearances of a figure. It is the ability, of course, to do these things in the absence of the printed figure. In other words, the fluctuation rate increases as the ideas of the two appearances become equally definite.

If the first principle is true, familiarity with what a figure represents should be a factor influencing the rate of change. We find that the 3 figures with the fastest rates all represent familiar objects, and the familiarity with the opposed interpretations of each figure is about equal. Figs. 7 and 8, which probably represent the least frequently experienced objects, have the slowest rates. Fig. 1 may be seen as stairs, and stairs are familiar objects, but there is an unequal familiarity with the two interpretations of this figure and we find that it has a rather slow rate. Fig. 5 represents a familiar object, but it also favors one interpretation over the other and if the second principle is true this would make the fluctuation rate slower. It may also be true that Fig. 3 leaves too much to the imagination of the *S*, so that the cortical responses are not what they otherwise would be. The fact that some *S*s have difficulty in keeping any perspective at all in this figure favors this explanation.

If the second principle is true, we should find a relatively slow rate of change whenever one interpretation of a figure is favored over another, and ordinarily

this will be the case whenever the *S* is more familiar with the figure as seen in one phase than he is with the figure as seen in the other phase. In the group of simple figures, we find that the two with the slowest rates (Figs. 3 and 8) are those in which there is the greater tendency for one interpretation to be favored over the other. In the group of complex figures we again find that the two with the slowest rates are those which favored one of the alternative interpretations.

Another fact that is of considerable importance is the rather rapid increase in the fluctuation rate to be observed from one day to the next. The work with 3 of our *S*s was interrupted by the Christmas holidays, and this fact complicated the results so far as this point is concerned because the rates for the various figures were slower the first sitting after the holidays than for the one just preceding the holidays. But the work with 3 *S*s was extended over 5 days in 5 successive weeks. The means for all figures and for these 3 *S*s, for successive days are: 7.49, 5.15, 4.12, 2.56, and 1.95. Many individuals when viewing some of these figures for the first time have to wait a long time before there is any change.[3] Introspections show that as the *S* gets clearer ideas of the appearance of a figure in each of its phases the rate of change becomes more rapid. As the *S* becomes better able, with the card removed, to describe or to image each of the two alternative appearances, the rate increases. With time, this ability with regard to the two appearances or interpretations becomes more nearly equal, and the rate increases.

Summary

(1) Using reversible-perspective figures, we found some evidence that complexity as indicated by the number of lines or other units has a tendency to make the rate of change slower. Other factors so modified the results, however, that the differences we found have but slight statistical significance.

(2) The figures representing the more familiar objects fluctuated more rapidly than those representing the less familiar objects unless there was unequal familiarity with the two interpretations of a figure.

(3) The fluctuation rates for all cards decreased rapidly during the course of the experiment.

(4) As a result of a detailed study of our objective and introspective data we discovered that two factors other than complexity were influencing the results. We believe that, other things being equal, the rate of change is proportional to the definiteness of the ideas involved in the two interpretations. We also believe that, other things being equal, the rate of change is proportional to the approach to equality in definiteness of the ideas involved in the two interpretations of a single figure.

[3]We did not begin taking records until the fluctuations were fairly regular.

THE STIMULUS CORRELATE OF BULKY COLOR

By Glenn A. Fry, Duke University

"We must distinguish," says Hering, "*roomy* (raumhaft) from *superficial* sensations. . . . A *roomy* sensation is that of clear and colored fluid in a glass; the yellow of the wine is seen not only on the bounding surface; the yellow sensation fills the whole interior of the glass."[1] "*Surface colors*," writes Troland, "seem to be confined to, and to constitute, the superficies of visual things. . . . The experience of a clear and transparent colored object, such as glass, frequently presents a volume or *bulky color*."[2] *Bulky colors*, according to Martin, differ from *surface colors* "in that they are tridimensional and partially transparent. They seem to fill a definite space in three dimensions."[3] "According to my observations," says Katz, "they show this property in a distinct way only when they are at the same time transparent,"[4] that is, so long as objects are actually seen through them.

The perception of either bulky or surface color, according to Katz, depends upon the influence of the *innere Einstellung*, and he designates "as almost irrelevant the source of radiation setting up the stimulus;"[5] that is to say, when color is referred to an object, it becomes a surface; when it is transparent so that objects may be seen through it, it becomes a bulky color.

The recognition of this aspect of color, the distinction between surface and bulky color, is an important contribution to the theory of visual perception; but should we accept Katz' psychological explanation, namely, that the "mode of color appearance" is due to the influence of the *innere Einstellung*, without first attempting to trace this aspect of color to some factor in the physical world, which might serve as an adequate stimulus correlate? One such factor, that has been overlooked, is the distribution of intensity; and this paper is devoted to a description of how disks, designed to represent different distributions of intensity, yield colors which differ phenomenally in *textural quality*,—some soft, fluffy and bulky, some hard and smooth.[6]

Surface color.—To begin with, a uniform distribution of intensity gives us the perception of a *hard, smooth color*, or as Hering would call it, a "*strictly surface*

*Accepted for publication November 19, 1930.

[1]E. Hering, Hermann's *Handbuch der Physiologie*, III, (1), 1879, 575.

[2]L. T. Troland, Visual phenomena and their stimulus correlations, *Foundations of Experimental Psychology*, 1929, 173.

[3]M. F. Martin, Film, surface, and bulky colors, and their intermediates, this Journal, 33, 1922, 453.

[4]D. Katz, *Die Erscheinungsweisen der Farben*, 1911, 17.

[5]Katz, *op. cit.*, 9.

[6]This discovery was made during a series of experiments with the phenomenon of graded contrast, whose stimulus correlate is a smoothly graded distribution of intensity. The phenomenon was discovered by Professor Wm. McDougall and is reported in the *Proc. Physiol. Soc.*, March, 1903, in an article entitled "Intensification of visual sensation by smoothly graded contrast."

color." A paper disk of uniform color, regardless of its texture or finish, when applied to a color rotator, gives a hard, smooth, surface color. Kymograph paper has a glazed finish as compared with the soft, dull finish of mimeograph paper; but if we cut a disk from each of the two varieties of paper and apply them to color rotators, we cannot distinguish any difference in textural quality, for each appears hard and smooth. A hard, smooth color may be obtained with a gray disk as well as with a white disk, or with a disk composed of black and white sectors, provided that the disk is rotated fast enough to eliminate flicker: in each case we get a uniform distribution of intensity.

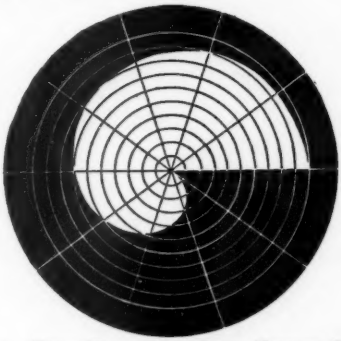


FIG. 1. DISK YIELDING A SOFT, FLUFFY COLOR

For demonstrational purposes the lines which serve as polar coördinates should be omitted. They are added here to show the construction of the disk.

Soft, fluffy color.—When the disk illustrated in Fig. 1 is rotated fast enough to give complete fusion of the black and white, it yields phenomenally a patch of color, *soft* and *fluffy* in texture. If a strong light is thrown upon the disk (*e.g.* with a projection lantern), the fluffy color appears to *glow* or be self-luminous; the degree of glow (*i.e.* the degree to which the fluffy color appears to be self-luminous) depends upon the intensity of the light thrown upon the disk. This correlation of glow with soft, fluffy, textural quality of color bears out the observation of Hering that *glowing* bodies give us a perception "which seems *roomy* as compared with that of *strictly surface color*. A glowing iron appears luminous through and through, and so does a flame."[7] In spite of the fact that the physical intensity is graded from white at the center to black at the circumference (as a glance at the disk will show), the patch of color appears almost uniform.

Bulky color.—The disk illustrated in Fig. 2, when rotated, gives phenomenally a patch of color, flimsy and insubstantial in texture, misty, vaporous, and bulky; so foggy that apparently nothing could prevent the observer from stirring it up with his finger. In spite of the fact that the physical intensity is almost smoothly graded from the center to the circumference, the patch of color appears almost uniform. This discrepancy between the physical and the phenomenal distribution of intensity is greater than in the case soft, fluffy color (Fig. 1).

[7]Hering, *op. cit.*, 572.

Relation of bulky to surface color.—Since the physical intensity at any point along the radius of the rotating disk depends upon the proportion of black and white mixed at that point, the distribution of black and white on the disk serves as an index to the distribution of intensity. This offers a basis for the comparison and classification of textural qualities, a classification according to the distributions of intensity with which they are correlated.

The equation of the curve which outlines the white figure on the disk which gives soft, fluffy color (see Fig. 1) is $\rho = \log (360° - \theta)/a$, where a is 36°. The equation of the curve which outlines the white figure of the disk which gives bulky color (see Fig. 2) is the same with $a = 240°$. A disk based in like manner

FIG. 2. DISK YIELDING A BULKY COLOR

upon the curve, with $a = 1$, would approximate a uniform distribution of intensity, which, as already pointed out, gives phenomenally a hard, smooth color. An infinite series of disks may be designed to demonstrate a corresponding infinite series of textural qualities, ranging from a misty vapor to a hard, smooth color. The pattern of each of this infinite series of disks would be based upon the general equation, given above.

I have tested and verified this generalization for six curves chosen at random, in which a is equal to 240, 90, 60, 36, 20, and 7.5 degrees respectively. There is correlated with every degree of textural quality a corresponding degree of discrepancy between the physical and the phenomenal distributions of intensity: the more marked the discrepancy, the more flimsy and insubstantial becomes the color.

Conclusions.—The distribution of intensity is an adequate stimulus condition which accounts for the phenomenal differences of textural quality between surface and bulky color, and their intermediates. Disks designed to represent different distributions of intensity yield colors which differ phenomenally in textural quality: some soft and fluffy, some bulky, and others hard and smooth.

The degree of bulkiness is correlated with a corresponding degree of discrepancy between the physical and the phenomenal distribution of intensity. This correlation suggests that a mere graded distribution of physical intensity is not in itself the ground for the perception of bulkiness, but only in so far as it gives rise to a redistribution of intensity values.

VISUAL SIZE AND DISTANCE

By T. H. Cutler, Purina Mills, St. Louis

It is well-known that the apparent size of visually perceived objects diminishes with the distance of the object from the observer, that the diminution, as distance increases, is not nearly so rapid as the diminution of the visual angle subtended by the object, and that the functional relation of apparent size to distance is not linear but follows a form that has been made out in the alley experiments.[1] The writer has undertaken certain preliminary experiments in this field, and he presents the results briefly here because they seem suggestive and because he is prevented from continuing work on this problem at the present time.

Procedure. *O* viewed a black curtain, 14 ft. in distance, through a rectangular box-like frame, which restricted the field of vision in all four directions and which also was arranged to serve as a head-rest. A standard and a variable stimulus were suspended at different distances from *O* in his field of vision and in such a manner that the nearer did not obscure the further. Each stimulus-object was mounted on cardboard with rings on its back. The rings could be placed on hooks on a pair of vertical black wires of such small diameter that they were invisible to *O*. These vertical wires were attached to frames that could be moved back and forth on horizontal wires stretched across the room; and the vertical wires were prevented from swaying by being weighted with pieces of chain that dragged on the floor. Only the middle of these wires and the middle of the curtain that served as a background were within *O*'s field of vision. The room was darkened during experimentation and the stimulus-objects were illumined by electric lights, above the field of vision and attached to the frames that supported the suspension-wires.

There were two kinds of stimulus-objects, a square and a 'hand.' The one was a brownish white square cut from cardboard; the other was a pen-and-ink drawing of a hand, with the shading designed to give it 'objectivity.' (see Fig. 1). The various sizes of the 'hand' were reproduced by photostatic enlargement from the same negative. It proved practicable to use the same variable stimuli at the different distances, but different sets of variables were required for different *O*s. Altogether it was necessary to have 14 sizes of each stimulus-object, varying in vertical dimension by half-inches from 7.5 to 14.0 in., besides the 6-in. standards.

Fig. 1. Drawing Used as One of the Stimulus-Objects

*Accepted for publication November 22, 1930.

[1]From the Harvard Psychological Laboratory, communicated by E. G. Boring. For the citations from the work of Hillebrand, Schubotz, Poppelreuter, and Blumenfeld on the alley experiment, see Boring, this Journal, 42, 1930, 311.

The method of constant stimuli was employed with five variables. The standard was always 6 in. high and always nearer to *O* than the variable.

O was instructed to "attempt to disregard all secondary clues (such as distance)" and to report any failures to follow this instruction. *B* reported the greatest difficulty in this regard, but his quantitative results are comparable to the results of the other *O*s.

There were three *O*s: W. A. Bousfield (*B*) and H. L. Kim (*K*), graduate students in the Harvard Psychological Laboratory, and an undergraduate M. E. Pitts (*P*), a senior who had had only an elementary course in psychology.

The limens were computed by Urban's procedure and the points of subjective equality were taken as the intersection of the two psychometric functions, for there were very few judgments of 'equal' and the equality cases were therefore divided equally between the other two categories. While the values of h varied from 1.0964 to 0.1002, all but five lay between 0.9976 and 0.5170. Table I gives the values of these points of subjective equality and also states the three pairs of distances used.

TABLE I

SHOWING THE POINTS OF SUBJECTIVE EQUALITY OF A VARIABLE WITH A 6-IN. STANDARD AT DIFFERENT DISTANCES OF VARIABLE AND STANDARD FROM THE *O*

Two stimuli, a square and a 'hand'. Three observers. All data in inches.

	Square				Hand			
Observer:	*B*	*K*	*P*	*Av.*	*B*	*K*	*P*	*Av.*
(*a*) Standard at 50 in.; variable at 100 in.	8.64	9.54	8.45	8.88	9.07	9.47	8.73	9.09
(*b*) Standard at 100 in.; variable at 150 in.	8.48	8.49	8.46	8.48	8.45	8.99	8.38	8.61
(*c*) Standard at 50 in.; variable at 150 in.	13.05	12.48	12.00	12.51	13.86	13.01	11.45	12.77
(*d*) Value of *c* implied by *a* and *b*.	12.20	13.50	11.91	12.55	12.77	14.19	12.20	13.04

Conclusions. While the method of constant stimuli is laborious, the consistency of the data indicates that it is an excellent method for the determination of the functional relation of apparent size to distance, and that elaborate apparatus (cf. the alley experiments) is not required for research in this field.

The table shows that 6 in. at 50 in. distance, is about equal to 9 in. at 100 in. distance, and that 6 in. at 100 in. is about equal to 9 in. at 150 in. The variations from this general statement are apparent in the table, but they are not large or consistent. Thus it may be said that, under the conditions of this experiment, an object at 50 in. must be increased to 1.5 times its objective size if it is to remain the same apparent size at 100 in. and that it must again have increased by the ratio 1.5 at 150 in. if it is still to be the same apparent size. If the same ratio of sizes holds for equal increments of distance, the curve of growth of the stimulus-

object for constant apparent size must be logarithmic, with a direction of curvature that is consistent with the results of the alley experiment. This function should be again tested more precisely.

The last row of the table gives a check on the consistency of the data. The meaning of these figures may be illustrated by reference to the first column. If 8.64 in. at 100 in. equals 6 in. at 50 in., and if 8.48 in. at 150 in. equals 6 in. at 100 in., then (8.48/6) 8.64 = 12.20 in. at 150 in. ought to equal 6 in. at 50 in., whereas by actual experiment we see that 13.05 in. at 150 in. equals 6 in. at 50 in. The differences between the values of rows *c* and *d* are not great, especially in the columns of averages.

This experiment was originally undertaken to test a belief that an 'objectified' stimulus would show less diminution of size in recession than a simple geometrical figure or a barely defined extent. Köhler has implied that there is practically no diminution in the size of a person as he recedes from an observer.[2] Köhler's discussion of this point shows little relation to the experimental literature on the subject,[3] but nevertheless it is possible that a very familiar solid object would resist shrinkage on recession more than do the unfilled extensions of the alley experiment.

The table shows no such difference between the square and the hand. Among the 9 observed points of subjective equality, 5 are greater for the hand and 4 less. If there be any generalized difference it is that the hand shrunk a trifle more on recession than the square. However, the point can not be regarded as settled until some more 'objectified' stimulus-object than this drawing of a hand has been tried. If small manikins or dolls could be reproduced, identical with each other in every respect except size, it would be worth while to try the experiment again.

[2]W. Köhler, *Gestalt Psychology*, 1929, 74-76; cf. also 86 f., 93 f., 105 f., 133.
[3]Cf. Boring, *loc. cit.*

MINOR STUDIES FROM THE PSYCHOLOGICAL LABORATORY OF THE UNIVERSITY OF NEBRASKA

IV. Learning to Inhibit and to Control Breathing

By Rudolf Vogeler and J. P. Guilford

The study of voluntary inhibition of reflexes has been a comparatively neglected field. Some work has been done upon the voluntary control of the heartbeat, body temperature, and the wink reflex,[1] but not enough even to permit the formation of any plausible theories. Just what connections exist between the functioning of the higher brain centers and the reflex centers when a reflex is brought under control is not known. Such reflexes as those governing the excretory functions, and a number of others, are undoubtedly brought under voluntary control through a process of training in the life of ever individual. The proper integration of these reflexes into the 'total personality' is probably very important from the standpoint of mental health. The recent works of Jacobson,[2] and others, tend to show the very great importance of voluntary control over the more automatic and unconscious processes as a psychotherapeutic measure.

The practical consequences to be derived from the ability to control breathing voluntarily are especially important for the fields of physical education and athletics. Cureton has pointed this out admirably in his work on respiration as related to speed and efficiency in swimming.[3] There is also the popular interest in such feats as those of the late Houdini in which he purported to have held his breath for a matter of minutes under water, and to have kept alive by a process of slow breathing while being buried under earth for a much longer time.[4]

Object

Our study has to do with the following points: (1) What is the average ability, before training, either to hold the breath or to breathe slowly, *i.e.* to breathe with a minimum of intake? (2) Can the average individual improve in either of these two tasks, and what sort of learning curves will result? (3) If one wishes to learn to hold his breath, is it better to practice slow breathing (partial inhibition) or to practice holding (complete inhibition)? It was thought that the last problem would have more general implications. That is, in learning to

*Accepted for publication November 25, 1930.

[1]G. E. Partridge, Control of the reflex wink, this Journal, 11, 1900, 244-250.

[2]E. Jacobson, *Progressive Relaxation*, 1929.

[3]T. K. Cureton, Relation of respiration to speed efficiency in swimming, *Res. Quar. Amer. Phys. Asso.*, 1, 1930, 54-70.

[4]H. Kelloch, *Houdini: His Life Story*, 1928, 4, 7, 256, 371. Kelloch states that the hand-cuff wizard practiced holding his breath until he was able to remain under water longer than four minutes. In the burial test he was able to remain in a coffin under water for about an hour and a half.

inhibit any action, should one do so 'by degrees,' or should one take more drastic action and practice complete inhibition?

Method and Procedure

A Sumner pneumograph was fastened about the chest of the subjects at the place where the greatest movement due to breathing was noticed. In order to insure at least approximately the same tightness for each practice session, the chain used for fastening was hooked into the same link each time. Kymograph records were made by means of a Marey tambour. A screen was placed between the subject and the recording apparatus to minimize any effect which the presence of the apparatus might have upon him. Material which the *S*s were instructed to read during each trial of the experiment was fastened upon the screen at about the level of the eyes. This reading material consisted of articles on topics of general interest, and short stories of an unemotional type, and served the purpose of centering *S*'s attention upon the reading rather than upon the breathing. In this way a more nearly correct normal breathing record was obtainable. Tests of vital capacity were given with a wet spirometer, at the very beginning of the study and every 5 days thereafter.

The training period lasted 3 weeks, every week consisting of five practice days followed by two idle days. Five practice trials were allotted to every *S* on every practice day, the first four being regarded strictly as practice, the fifth as the test trial. In this way the first four trials of Group A were 'holding' practice trials, the first four trials of Group B were 'slow breathing' practice trials, and the fifth trial for both groups was a 'holding' trial. This allowed a comparison between the two groups to determine the effect of different kinds of practice upon the fifth, or 'holding' trial.

Ten subjects divided into two equal groups, A and B, were used. Group A practiced 'holding' entirely and Group B practiced 'slow breathing' with the fifth and last trial each day of 'holding.' Prior to the start of the actual training period, a practice session to determine normal breathing was given. This period varied from 3 min. in some cases to 20 min. in others. A 3-min. normal breathing trial was also given every day before the practice trials were begun. These precautions served the purpose of acclimating the *S*s to the use of the apparatus and the usual conditions of the experiment, not only during normal breathing, but also during 'holding' and 'slow breathing.'

Instructions. The instructions for 'holding' were as follows.

When I say 'begin' you are to start reading silently the paper in front of you. Read at your normal rate. Breathe normally. When I say 'hold your breath' you are to stop breathing instantly and hold your breath as long as you can. Continue to read at your normal rate but remember that you are not to breathe. When you can no longer hold your breath, begin breathing as naturally as you possibly can and continue thus with your reading until the end of the experiment.

The instructions for 'slow breathing' were as follows.

When I say 'begin' you are to start reading silently the paper in front of you. Read at your normal rate. Breathe normally. When I say 'breathe slowly' you are to reduce your breathing to a minimum at once. Breathe as little and as slowly as you can without stopping. Continue to read until the end of the experiment but remember to breathe as little as possible.

The point at which the signal to 'hold' or 'breathe slowly' was given, was recorded on the drum and a stop-watch was used to time the length of the 'holding' or 'slow breathing.' A quick and long inhalation at the end denoted the completion of the trial. *S* was informed of his success in terms of seconds after each trial.

The two groups of *S*s were selected so as to be as comparable as possible. They were approximately equal, man for man, so far as 'type' (athletic or student) and weight were concerned. They were also very close together as regards vital capacity. The average for Group A was 262 cu. in. and for Group B, 242 cu. in. The difference is only 20 cu. in., or 8.1% in favor of Group A. The vital capacity proved not to be an important item, since it correlated neither with the depth of normal breathing nor with the initial ability of the individual to hold his breath. The depth of breathing was measured from the pneumograph records by selecting 5 typical breathing curves on the kymograph record of each day and obtaining an average. The averages were 2.40 cm. and 3.27 cm. for Groups A and B respectively. Here Group B has an advantage of 36.3%. This difference is probably great enough to be significant. It may account for the fact that Group B had a distinct initial advantage over Group A in holding the breath. It is probably not an indication of the relative general physical capacities of the two groups, for the depth of breathing was uncorrelated with the vital capacity and the latter is recognized as being a better indicator of general physical capacity. Holzinger's work on relations between vital capacity and what he terms "dynamic" and "static" physical characteristics shows a very slight positive correlation.[5] The importance of the difference in depth of breathing may therefore be somewhat discounted.

Results

Among the results, let us first notice the rate and degree of improvement in the two different tasks, 'holding' and 'slow breathing,' comparing the two groups of *S*s as to initial ability, rate of learning, and final accomplishment. We need examine only the first 4 trials of every day's record for this; the fifth and final trial was a test trial in 'holding' for both groups.

It was not easy to establish a satisfactory criterion for initial ability in either group. The first day's records, based upon only 4 measurements for every *S*, were not considered reliable enough. An average was found for the first 3 days for each group. These were 31.6 sec. and 78.6 sec. for Groups A and B respectively. But there were daily variations of considerable size, so in order to secure a more reliable measure of initial ability, an average of the first 7 days was also computed. This was 40.5 from Group A and 99.4 for Group B.

That the initial ability for 'holding' should be less than the initial ability for 'slow breathing' is easily conceivable when one considers the outstanding differences between the two types of practice. A *complete inhibition* over a prolonged period of time was necessary for the 'holding' group as compared to an incomplete or *partial inhibition* for the 'slow breathers.' It seems reasonable to suppose that a complete inhibition would be more difficult to sustain voluntarily than a partial one, especially in the case of breathing where a decrease of oxygen,

[5] K. J. Holzinger, Note on relation of vital capacity to certain physical characters, *Biometrika*, 16, 1924, 139-156.

plus an increase in carbon dioxide, plus atmospheric pressure, almost demands the successive contractions and expansions of the diaphragm and the accessory breathing muscles. Considering the above difference from the point of view of the physiological breathing requirement, it meant that the *Ss* in Group A had to adapt themselves to a complete cessation of oxygen intake, whereas those in Group B needed merely an adaptation to a minimal amount of oxygen. Con-

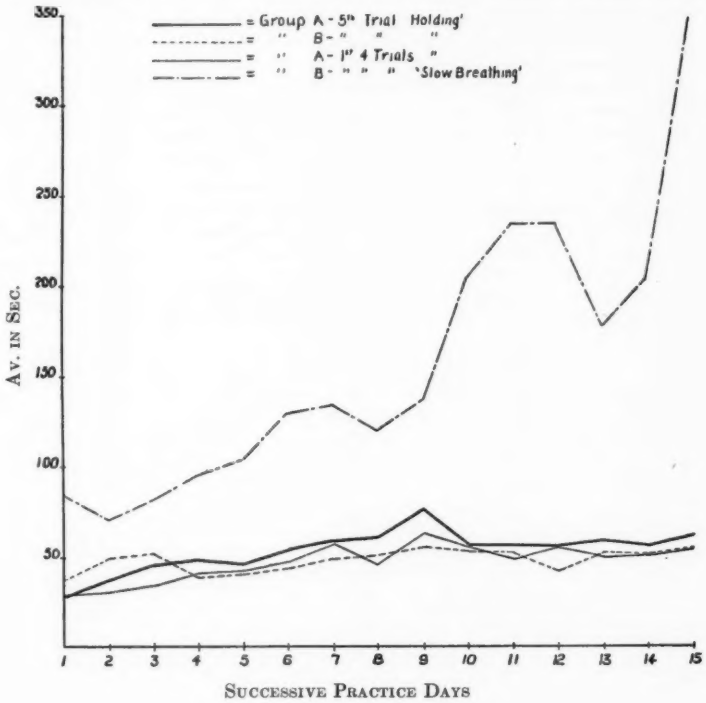


FIG. 1. FORM OF THE LEARNING CURVES

sequently the difference in initial ability of 145% in favor of 'slow breathing' is easily explained.

Fig. 1 shows the general form of the learning curves. For the first 9 days the trend of the curves indicates a rather steady increase in ability, but from that point on the 'holding' group drops a trifle with a subsequent flattening out of the curve, whereas the 'slow breathing' group continues to increase even more rapidly then before. The members of Group A may have reached a plateau in their ability to 'hold,' or they may have reached their physiological limit. We are

inclined to believe that it was only a plateau and that improved methods of 'holding' would have given a new impetus to their learning. Group B shows a marked acceleration, and we cannot predict when it might have come to an end. This rapid improvement might be explained by Ogden's idea of "aesthetic appreciation."[6] In this phenomenon the *S* spends his time in developing methods during the early practice periods. At some time during this training, the feeling of doing the task "right" is encountered. From that time on, a repetition of the correct movement results in a rapid increase in ability to perform the act more efficiently. This explanation is still further borne out by the fact that several *S*s reported as follows: "It almost becomes automatic." "It gets easier every time." "It is easier to do when I smoothe out the breathing." The latter "smoothness" would certainly indicate a greater efficiency in muscular effort.

TABLE I

SHOWING FOR THE TWO GROUPS OF *S*S THE IMPROVEMENT IN 'HOLDING' AND IN 'SLOW BREATHING' DURING THE PRACTICE TRIALS, AND IN 'HOLDING' DURING THE TEST TRIALS

(Time in sec.)

		Group A		Group B	
		3-day av.	7-day av.	3-day av.	7-day av.
Practice trials	Initial ability	31.6	40.5	78.6	99.4
	Ability after practice	51.9	54.2	241.6	218.9
	Amount improvement	64.2%	33.8%	207.3%	120.2%
Test trials	Initial ability	36.7	45.6	45.9	43.9
	Ability after practice	58.5	59.9	59.9	51.7
	Amount improvement	59.7%	31.4%	31.4%	17.9%

It will be observed that the 'holding' curve dropped a little and then rose more gradually. This phenomenon is very often encountered in athletics and physical education. Learning the 'kip' on the horizontal bar will serve as an example. A naïve *S* may repeatedly practice the desired coördination and more nearly approach the desired goal each time, until he finally performs the task. Very often then, he will "lose" it, as a gymnast would express the sudden inability to perform the stunt. The *S* does not return to the initial stage of incoördination, but rather to some intermediate stage acquired in the learning process. From that time onward, however, he increases his ability to "come back," or to perform the finished product faster than formerly, and then retains it indefinitely.

In order to determine the final abilities in 'holding' and in 'slow breathing,' we again used two criteria; the averages of the last 7 days and of the last 3 days. For Group A, the final averages were 54.2 sec. (7-day average) and 51.9 sec. (3-day average). For Group B the averages were likewise, 218.9 and 241.6 sec., respectively. Whereas the *S*s of Group B can breathe slowly only about 145% longer at the start than Group A can hold the breath, in the end they have an advantage of 304% based upon the 7-day averages.

[6] R. M. Ogden, *Psychology and Education*, 1926, 139.

The difference in the ability after practice between the two groups is a little more than double the difference between them in initial ability. This difference is borne out by the rate of improvement for both types of practice, the amount of improvement for 'holding' being 33.8%, compared to 120.2% for 'slow breathing." A possible explanation for this increased difference may be obtained from the physiological results of the *Ss*' reports.

It was noticed that yawning and drowsiness accompanied 'holding' quite frequently. These two factors ordinarily accompany a general relaxation of the entire musculature of the body. Due to this relaxation a more complete inhibition might ensue. For the 'slow breathing' group the fatigue factor was more evident. It seems that this naturally would accompany 'slow breathing' because the task is analogous to one of conditioning a muscle. Not only were these muscles (the diaphragm and accessory breathing muscles) being conditioned, but they also were being exerted for prolonged periods, which would result in fatigue.

To return to the difference in the ability of the two groups, it is evident that relaxation does help complete inhibition, but not to the extent that it compensates for entirely depriving the body of oxygen. But conditioning a muscle to act in a diminished capacity for a prolonged period of time might compensate for a reduced amount of oxygen intake, especially if the rate of breathing were increased. This latter phenomenon is noticeable in many of the kymograph records.

What was the effect of the two kinds of practice upon the ability to 'hold?' To answer this question, we must look to the data upon the final or test trial of each day and compare the two groups in this respect. In Table I it will be seen that for the first 3 days Group B has a distinct advantage which is overcome, for the 7 day averages give Group A a slight advantage, (3.6%). Fig. 1 shows the progress of the two groups in this respect. Group A gains and keeps its superiority in 'holding' to the end of the practice period. For the last 3 days it has an average advantage of about 6 sec. (see Table I). In view of the fact that Group A began with a 9-sec. disadvantage, the rates of improvement in 'holding' in the two groups are very significantly different.

A question might well be raised as to the reliability of averages based upon only one trial a day, the fifth or final trial. Some indication of this reliability is gained from the correlation which was found between the time of this final trial and an average of the first four for the same day. With 75 of such pairs of measures (in Group A, 5 *Ss* for 15 days) the Pearson product-moment coefficient of correlation was 0.88. Of course this correlation is spurious to the extent that improvement has taken place in both the four practice trials and the final test trial, but an examination of the two learning curves in Fig. 1 indicates that the high coefficient is not entirely due to learning. In view of this, the fifth trial may be taken as a reasonably good test trial for both groups.

It is impossible to conclude what results would have been obtained had the practice period been extended. From the appearance of the learning curves of both groups (Fig. 1), it is reasonable to suppose that neither group had attained its maximum ability. The tendency of the last four days in both groups is cer-

tainly toward an increase. It is also reasonable to suppose that a process as vital as breathing cannot be commanded through voluntary control as opposed to the accompanying physiological requirements in the short space of 15 days.[7]

Conclusions

(1) The average ability to hold the breath before practice was about 30 sec. The average length of time for slow breathing was about 80 sec.

(2) The improvement in 15 days of practice was about 65% in holding and 207% in slow breathing.

(3) The final ability in holding the breath was about 52 sec., in slow breathing approximately 242 sec.

(4) The ability to hold the breath is improved more by practicing holding than by practicing slow breathing. The two tasks probably involve two distinct types of training.

[7]The best single record in 'holding' was 2 min. 15 sec., in 'slow breathing' 12 min. 27 sec. These are somewhat short of Houdini's purported records, but they indicate that the latter's record might be approached, if not surpassed, by the average man.

MINOR STUDIES FROM THE PSYCHOLOGICAL LABORATORY OF STANFORD UNIVERSITY

II. Color Preferences in Terms of Sigma Units

By Paul R. Farnsworth and T. L. Chichizola

Guilford has developed a method by which preferences for various stimuli can be expressed in sigma units.[1] In this procedure the method of paired comparisons must be employed. Guilford has found that his method correlates quite well with that devised by Thurstone a short time ago.[2] The writers have employed the Guilford method to test the color preferences of 125 *Ss*, boys of the Preston School of Industry at Ione, California.[3] The stimuli were 2-in. squares of Munsell colored papers: no. 12, maximum yellow; 36, maximum purple; 8, maximum yellow red; 20, maximum green; 32, maximum purple blue; and 4, maximum red. These papers were presented in a two-window tachistoscope at approximately the speed of 3 exposures per min.

Fig. 1. Values of Color Preferences in Single Units

The values in sigma units were found to be as follows: red, +0.80; purple blue +0.32; green, −0.07; yellow red, −0.18; yellow, −0.28; and purple, −0.52 (Fig. 1). This order is very similar to that reported in many earlier studies.[4] It has been generally thought that boys prefer blue to red, while girls rate red above blue. The boys of this study did not show such a tendency.

*Accepted for publication October 1, 1930.

[1] J. P. Guilford, The method of paired comparisons as psychometric method, *Psychol. Rev.*, 35, 1928, 494-506.

[2] L. L. Thurstone, A law of comparative judgment, *ibid.*, 34, 1927, 273-286.

[3] The writers wish to acknowledge the coöperation of Superintendent O. H. Close of the Preston School of Industry.

[4] A. R. Chandler, Recent experiments on visual aesthetics, *Psychol. Bull.*, 25, 1928, 720-732.

III. Further Data on Suggestion in Pictures

By Paul R. Farnsworth and Issei Misumi

In a previous study it was demonstrated that Stanford students could be influenced in their judgments of pictures by descriptive paragraphs which were attached to the picture mats.[1] Two paragraphs or squibs were constructed for each picture. One squib considerably praised the picture; the other gave less praise.

The present paper reports a somewhat similar attempt to find the influence of artists' names on the preference value of pictures. To find what artists were well-known a list of 50 names was given to a large group of students in elementary psychology. These *S*s were to check all familiar names, and to double-check the 4 best known. By both of these methods the 4 best known were found to be da Vinci, Rembrandt, Raphael, and Rubens, while the 4 least known members of the list were Doughty, Kensett, Dewing, and Smibert. This finding was verified when the questionary was given to another large group of students.

As in the former experiment, a rating scale of 1 to 5 was used (1 meant 'best'). Every *S* was given a rating sheet which was arranged with the directions at the top, and with rating spaces as in the following example: "Picture No. 1 by Smibert ———— (put rating here)." Two groups of *S*s, students in Stanford University, viewed each picture, which had one of two names attached. That is, one group viewed all the prints, some of which had well-known names attached, while others had unknown names. The second group rated the same pictures which, this time, had unknown names where the former groups had well-known names, and vice versa. Only a few students had had sufficient artistic training to suspect that the names were not authentic. Their data were, of course, held separate. The following table gives certain portions of the data.

Well-Known Names	M	σ	N	Unknown Names	M	σ	N	$D/\sigma_{diff.}$	Chances in 100
Raphael	2.61	.97	89	Smibert	2.82	1.01	56	1.24	89
Rubens	3.19	1.09	56	Smibert	3.54	1.16	89	1.84	96
Rembrandt	3.23	.92	89	Dewing	3.46	1.00	56	1.44	93
Raphael	2.08	1.01	56	Dewing	2.47	1.09	89	2.17	99
da Vinci	1.62	.82	56	Doughty	1.83	1.04	89	1.40	92
Rembrandt	2.01	.88	56	Kensett	2.22	1.05	89	1.31	90
Rubens	1.99	.88	89	Kensett	2.00	.85	56	.07	53
da Vinci	3.37	1.04	89	Doughty	3.80	.92	56	2.69	100

Although the $D/\sigma_{diff.}$ values are not of sufficient magnitude to satisfy the criteria of all statisticians, they would appear to indicate that artists' names have some potency in the preference value of pictures.

[1] P. R. Farnsworth and H. Beaumont, Suggestion in pictures, *J. Gen. Psychol.*, 2, 1929, 362-366.

IV. A Case of Unusual Ability in Simultaneous Tapping in Two Different Times

By Paul R. Farnsworth and W. F. Poynter

For some time the senior author has been interested in so-called ultra-modern music. Of particular interest have been the rhythms employed, especially where one hand plays in 3/4 time, for example, and the other in 5/4. Mr. Arthur Hardcastle, an ultra-modern composer and performer, was recently prevailed upon to devote an afternoon to a series of tests. He was chosen since he is one of the few musicians who is known to play music which contains uncommon rhythms.

A modified Weiss-Renshaw polygraph was employed for recording the taps.[1] Mr. Hardcastle tapped telegraph keys (one in each hand). The set-up gave conditions far different, of course, from those occurring in the midst of a musical performance. For this reason the *S* was somewhat reluctant to undertake the experiment.

The taps ranged in speed from about 2 to 7 a second. (A slower speed seemed to excite the *S*.) The test began with the left hand tapping 2 beats to the right hand's 3. Then came 3 against 4, 3 against 5, 3 against 7, 4 against 5, and 4 against 7. The *S* was allowed a practice period in which to satisfy himself that he was actually tapping the desired rates. In every case the left hand took the slower tapping rates. Little accenting occurred.

The results seemed surprisingly good. To the unaided eye the records appeared to be machine tapped. In no case was there an error of over 1/10 the distance from one tap to the next. It was thus seen that Mr. Hardcastle can tap quite well the complicated times he professes to employ in his musical work.[2]

[1] S. Renshaw and A. P. Weiss, Apparatus for measuring changes in bodily posture, this Journal, 37, 1926, 261-267.

[2] Since writing this report an analysis has been made of the performance of Henry Cowell, and a survey has been made of the training methods employed by music departments to aid pupils in meeting such cross rhythms. A new method of training has been developed as a result of these studies.

APPARATUS

A TEMPORAL FINGER MAZE

By Roger G. Barker, Stanford University

This note describes an application of the principle of Hunter's temporal maze for animals to the human finger maze.[1] The path in the present instrument is grooved out of 4/16 in. battleship linoleum; it is 1/2 in. wide and 3/16 in. deep. The path pattern is sketched with a pencil; this outline is traced carefully on both sides with a sharp knife cutting into the linoleum to a depth of 1/16 in., and a groove chisel is then used. The path is worked over with the chisel to make the curve at the bottom of the trough uniform and smooth, and it is finished with an application of floor wax. The pattern selected and its relative dimensions are indicated in Fig. 3.

The blindfolded subject starts at point S, Fig. 3 (see also the position of the subject's finger in Fig. 2); this point is indicated to the subject by a small tack head. He proceeds toward the center of the intersection, B, which is indicated by a round-headed upholstery tack. From the center of the intersection, the subject may proceed by one of the four radiating paths, 1,2,3, or 4 (Fig. 3); he has to find the correct one by motor exploration. This correct path is the only one not blocked by a wire post, 1/8 in. in diam. and 3/16 in. high. These posts may be removed at the will of the experimenter by manipulating the levers I, II, III, and IV. The levers extend beneath the board panel, and are hinged to it at the end near the subject (Fig. 2). Holes 3/16 in. in diam. are made in the center of each radiating path 1½ in. from the center of the intersection. One of the levers extends below each of these holes and carries a metal post so placed that when the lever is against the panel (*i.e.* not pressed by *E*) the post will project above the trough of the path 3/16 in. The levers are held against the panel by silently acting springs. Thus, all paths are closed until *E* opens one by depressing a lever.

E depresses the levers in accordance with the formula for the particular pattern *S* is learning. If the problem requires that path 1 be learned first, *E* depresses lever I and holds it down until *S* finds his way out over the peg hole. *E* then allows the spring to return the peg, and depresses the lever corresponding to the next correct choice in the pattern. It is only necessary for *E* to have before him the numbers indicating the order in which the levers are to be depressed. Obviously it is possible to make such a maze with any number of radiating paths.

Certain advantages enumerated in another connection by Miles,[2] with whom the writer has worked on these problems, apply even more strongly to this tem-

[1] W. S. Hunter, The temporal maze and kinaesthetic sensory processes in the white rat, *Psychobiol.*, 2, 1920, 1-17; and The behavior of raccoons in a double alternation temporal maze, *Ped. Sem.*, 35, 1928, 374-388.

[2] W. R. Miles, Unidirectional mazes for studying human learning, *J. Gen. Psychol.*, 5, 1931, 269-275.

poral maze: (1) it is light and compact; (2) the pattern is easily and exactly described; (3) a very large number of maze patterns are immediately available, the same piece of apparatus serving for all the patterns; (4) successive choices in one direction does not carry *S*'s hand far to one side predisposing to choices in the opposed direction; and (5) *S* may be allowed to see the paths with all pegs in place before he begins, he thus understands exactly the nature of the situation, but learns nothing of the details of the choices to be made. This last point is of especial importance. If the maze is to become an accurate scientific instrument, a method is needed for controlling the amount of knowledge of the situation which the *S* has before he begins. Otherwise it is impossible to make comparable studies. In the past, the extent to which the nature of the situation has been revealed to *S* before he began his trials has varied from complete ignorance of the path pattern to more or less complete knowledge. It is evident that in the former case two distinct activities are involved: (*a*) that of exploring and identifying the nature of the situation to be learned; and (*b*) that of learning the situation after it has been identified. Small differences of accuracy in guessing the situation or in the amount of knowledge possessed by *S* may account for large differences in error, time, and trial scores. For example, if one *S* knows that at each choice point there will be three possible ways to proceed, he has a distinct advantage over one who only knows that some of the paths will be blocked. Or, the *S* who has discovered that the general direction of the true path is counterclockwise has an advantage over the one who only knows that the goal is somewhere within reach of his arm. It seems practically impossible so to standardize a verbal procedure that it can be universally trusted. Visual presentation is better and the temporal maze here described will permit of this.

In order to make a comparative study of the temporal finger maze and the spatial finger maze, the form illustrated in Fig. 1 was constructed. This maze duplicates the temporal maze in type of path construction and in type and size of intersection. It also could be shown by arranging the paper covering as illustrated in Fig. 4. The significant elements (the intersections) are shown through the holes, but the connecting paths for the correct exits are covered. The following instructions which were given to the *S*s describe its use.

On this board, I have grooved a path running from the bottom to the top. The path passes through the intersections you see through the holes in the paper covering. By observing the intersections now you cannot tell which is the path, for only one of the possible four exits leads on to the next intersection; the other three are blocked by pegs; they do not lead on. Now, I am going to blindfold[3] you and place your finger on the starting point and ask you to find your way to the upper end. The first time it will be a matter of chance which paths you take at an intersection; you will, of course, get into many blind exits. When you have reached the upper end, I will place your finger on the start again and you will try it once more. You will repeat these trials until you have learned to travel the true path from the bottom to the top without making a single error. When you make the trip perfectly three times out of four attempts in succession, we consider that you have learned the path.

It is possible to duplicate in the temporal maze the order of the correct exits from the successive intersections as they exist in this spatial maze. Four groups

[3]The standard blindfold was used; cf. W. R. Miles, The high relief finger maze for human learning, *J. Gen. Psychol.*, 1, 1928, 10.

of comparable *Ss* from the elementary psychology classes at Stanford University were used as follows: Group 1, 17 *Ss*, learned 10 intersections of the spatial maze; Group 2, 17 *Ss*, learned the first 6 intersections of the spatial maze; Group 3, 16 *Ss*, learned 10 intersections of the temporal maze with pattern comparable to the spatial maze learned by Group 1 (paths were opened in the order 4, 2, 1, 4, 3, 1, 3, 4, 1); Group 4, 63 *Ss*, learned 6 intersections of the temporal maze with pattern comparable to the spatial maze learned by Group 2 (paths were opened in the order 4, 2, 1, 4, 3, 1).

The reliability of these maze situations as obtained by finding the product-moment correlation of errors made on each blind on the odd trials with the errors made on the even trials (neglecting the first trial to allow for orientation) is as follows: 10 intersection, spatial maze (Group 1), 0.96 ± .02; 6 intersection, spatial maze (Group 2), 0.93 ± .03; 10 intersection, temporal maze (Group 3), 0.95 ± .02; 6 intersection, temporal maze (Group 4), 0.90 ± .03.

The relative difficulty of the mazes stated in terms of the mean number of errors per blind was found to be: Spatial maze, 10 intersections (Group 1), 4.87; Spatial maze, 6 intersections (Group 2), 3.91; Temporal maze, 10 intersections (Group 3), 1.75; Temporal maze, 6 intersections (Group 4), 1.43.

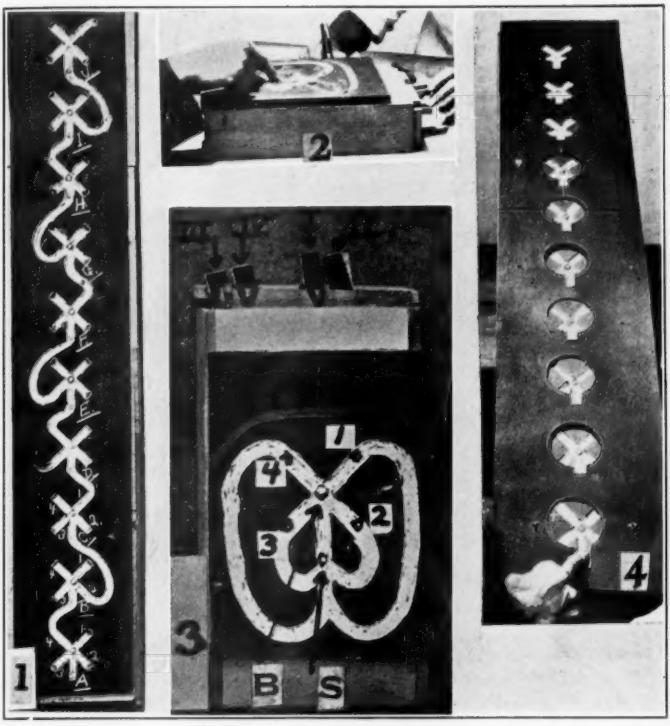
These data make it evident that the temporal maze presents a much more easy learning problem than does a spatial maze of comparable pattern. It has been a mooted question as to just what a person learns when he learns a maze. Some experimenters have considered it a motor learning problem, others, a verbal learning problem. The above data intimate that whatever is learned is not common to both the spatial and temporal mazes.

This suggestion is borne out by the low correlation obtained between the number of errors made upon corresponding blinds of the 10 intersection spatial and temporal mazes. The product-moment r is 0.10 ± .14. These mazes are identical in respect to the direction of the correct exit from each succeeding intersection. If it were true that this were the significant material learned, a high correlation would be expected between the frequency of errors on corresponding blinds. But this is not the case.

Introspective reports reveal that it was particular pattern elements which were learned rather than the universal pattern elements of 'rightness,' 'leftness,' 'upness,' and 'downness.' Such reports as the following were common: "The first one was a little bend, the second, a big hook, the third, a little hook," etc., (referring to the spatial maze). "First there was a big circle this way; then a little one that way;" "they are like clover leaves; first I went around the top leaf;" "it was like a butterfly with four wings;" etc., (referring to the temporal maze).

This fact that the particular pattern idiosyncracies were learned rather than the common, universal concepts of direction is emphasized by the fact that of 22 *Ss* who learned the spatial maze immediately after having learned the temporal maze, only 3 recognized the similarities between them; and of 16 *Ss* who learned the temporal maze after having learned the spatial maze none recognized the similarities.

The configurations of the pattern which most naturally lend themselves to easy generalization appear to be the significant context for most learners.



FIGS. 1-4. GROOVED MAZES TO BE TRACED BY THE FINGER
(The paths have been made white for purposes of illustration.)

FIG. 1. Unidirectional finger maze with three blinds and the true path opening from every intersection.

FIG. 2. Temporal maze in process of being solved by an *S* who is seated on the left. *E*, by pressing the keys, presents the choices equivalent to the sequence in Fig. 1.

FIG. 3. Scheme of the temporal finger maze.

FIG. 4. The maze illustrated in Fig. 1 covered by cardboard screen so made that *S* may be shown the general arrangement of the maze without learning the true choices.

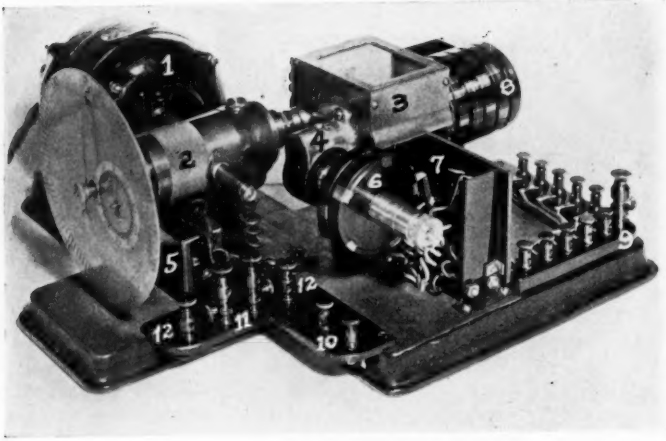


FIG. 5. COMBINED CHRONOSCOPE AND INTERVAL-TIMER

A COMBINED CHRONOSCOPE AND INTERVAL-TIMER

By Samuel Renshaw and I. L. Hampton, Ohio State University

The need for a reliable and simple chronoscope and interval-timer led to the design and construction of the instrument illustrated in Fig. 5. The main features of the apparatus are as follows.

(*a*) It is driven by a G.E. synchronous motor (1) which runs directly from a 60-cycle A.C. single-phase current outlet. The usual city current is of constant frequency within about 0.02%. This motor has a self-starting coil and runs continuously during the use of the instrument. The A.C. 'hum' is not loud enough to interfere in most experiments. The motor speed is 1800 r.p.m. A 30-to-1 worm reduction gives a main-shaft speed of 1 r.p.s.

(*b*) A specially designed magnetic clutch (2) couples the index hands upon the dial with the power source. The large hand makes 1 revolution per second clockwise around a scale divided into 100 divisions. The small hand, through a spur gear step-up, makes 10 revolutions to each one for the larger hand. Hundredths seconds are read directly from the large dial, and thousandths are read by taking the first figure of the large dial and following this by the two figures of the small dial. The hands may be reset to zero by inward pressure and manual rotation of the knurled nut holding the large hand. This clutch is silent and powerful. It is simple, being of the one-way type, and engages magnetically by compressing the armature against a helical steel wire spring. The hands stop rotating when the clutch magnet circuit is broken, releasing the spring. The throw of the armature is set at 0.002 inch.

Checked against a G.E. Type PM-12-A1 oscillograph operating at 500 cycles this chronoscope showed a constant starting lag of 0.004 sec. and a stopping lag of 0.005 sec.; hence there is a constant error of 0.001 sec. in the readings. The mean variation of several hundred readings checked against a pendulum make-break contactor was less than .0005 sec. We have never been able to attain this constancy and accuracy with either the Hipp or the Dunlap chronoscope.

(*c*) Six volts of D.C. operates the clutch. For the timing of responses, shutter-openings, etc., a specially built relay (5) closes the clutch circuit through the carbon-brush slip-rings attached to the clutch (2) and makes the signal circuit through the outlet binding posts (10). Binding posts at (11) are for the D.C. source and at (12) are the *E*'s control circuit. If desired, the relay may be omitted by operating the clutch directly from the terminals of the carbon-brushes attached to (2).

Once adjusted the instrument is virtually foolproof. All 'vital' parts are housed and protected from dirt and dust. There is no 'cranking' or phase hunting. It is practically impossible to 'slip' the clutch. Its gripping surface is the full 360°. The clutch magnet is contained in a split housing to avoid stray eddy currents. The constancy and accuracy of performance points to the correctness of the design.

(*d*) The main shaft is coupled directly to the 3-speed transmission encased in (3). The gear ratios are 1, 2, and 4, so that the keyed spindle bearing the slotted timer-commutator disks (8) may be rotated at speeds of 1, 2, or 4 r.p.s. A selective

shifting lever mounted on the reverse side of (3) makes quick change possible. This feature multiplies the function of each disk by 3. For example a 12-point disk can be made to interrupt a circuit 12 times, 6 times or 3 times per second, by shifting the gears in (3). Narrow silver brushes (7) are held by spring tension in apposition with the disk faces. Depression keys (not shown) allow for rapid change of the interval output through the binding posts (9). From 1 to 10 separate circuits can be cared for, and a very large number of rhythm patterns can be obtained by commoning two or more of the binding posts to the same circuit outlet. From the countershaft of (3) a further 30-to-1 worm reduction (4) gives to spindle (6) a speed of 1 revolution in 30, 60, or 120 sec., depending on position of the gear shift lever back of (3). This is for intervals of longer durations. Disks are identical for both shafts.

(*e*) Either the chronoscope or the interval-timer may be used independently. Additional disks, in any desired pattern of make-break intervals can be made for special experiments. The interval-timer can be coupled with the polygraph recorder and these two units made to constitute a ready means for constant temporal control in a wide variety of experiments.

AN APPARATUS FOR THE STUDY OF THE RELATIVE RÔLES OF PHASE AND INTENSITY IN SOUND-LOCALIZATION

By Otis C. Trimble, National Research Council Fellow, Harvard University

Investigation of the rôles of wave-phase and acoustic intensity in binaural sound-localization involves a complicated set of experimental conditions. Investigation of phase-relations requires that the intensities at the two ears be held equal while the phase-incidences at the two ears are varied in measurable increments or decrements. On the other hand, investigation of intensity requires that the phase-differences for the ears be held constant while the binaural intensities are varied. Experimental study of the effectiveness of either of these factors requires that the tones used as stimuli be at least relatively pure in order that conditions too complex for attentional analysis may be avoided. Furthermore, if such a study is to be complete it is necessary to be able to vary the frequency of the total binaural stimulus-pattern over the entire audible range.

The apparatus described here has been developed[1] to meet these conditions. The arrangement of the different control-devices is such that the different factors can be controlled accurately, conveniently and independently.

The apparatus consists essentially of an audio-frequency inductor-alternator (ABCDEIJKLMN), a tone-phaser (ABCILMN), a tachometer (F), and an electric motor (G), mounted on a common base (H). Figs. 6-8 illustrate the arrangement of the different parts.

[1]The writer desires to acknowledge his indebtedness to Professor Fred E. Dace, head of the department of electricity in Bradley Polytechnic Institute, to Mr. B. V. Nagasherv, an electrical engineer, graduate student in Harvard University, and to Mr. Ralph Gerbrands, technician in the Harvard Psychological Laboratory, for their assistance and advice in the design and construction of this apparatus.

The alternator consists of a rotor (D) which carries inductors, the teeth between the slots (J), a field coil (E) common to all of the inductors, and two separate, movable armatures (ABCILMN) which constitute the tone-phaser.

The rotor, which consists of a steel disk machined and ground with slots cut radially and at regular intervals near the rim, is mounted to run true. It is held

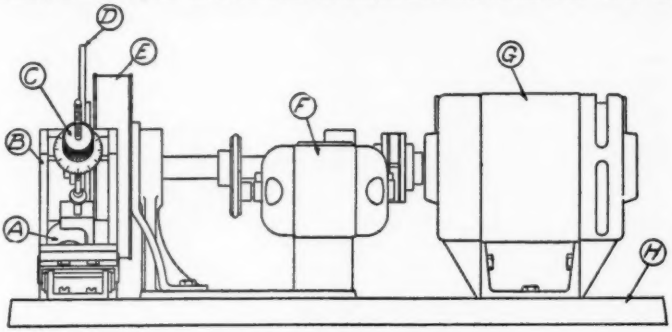


FIG. 6. SIDE VIEW OF TONAL APPARATUS

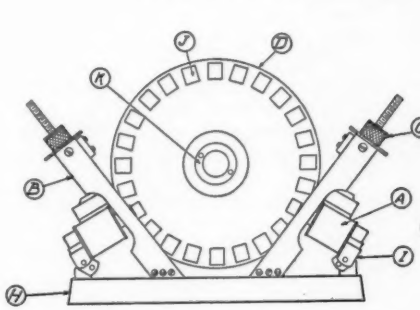


FIG. 7. END VIEW OF TONAL APPARATUS

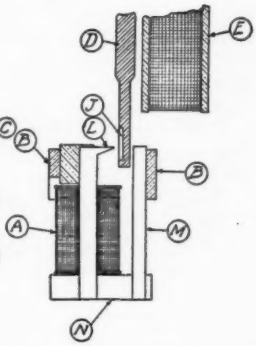


FIG. 8. SECTION OF ALTERNATOR

(A, armature windings; B, guides; C, screw-device; D, rotor; E, field coil; F, tachometer; G, motor; H, base; I, hinged supports; J, slots; K, hub-nut; L, armature core; M, brace; N, support.)

in position on the projecting end of the drive-shaft by means of heavy iron washers and a hub-nut (K). This arrangement makes it possible to use interchangeably disks of different construction. In the present set-up two different disks are used, one with 22 teeth for the generation of low frequencies and another with 104 teeth for the generation of high frequencies.

The field coil consists of 1500 turns of no. 24 insulated wire wound on a spool of brass. This coil is supplied with direct current from storage batteries.

The armatures are identical in construction (Fig. 8), each consisting of a core (F) of fifteen laminations of silicon-iron, and a coil (A) of 3000 turns of no. 30 enameled wire. They are mounted as separate units, each being supported by a system of guides, (B,M,N, and a pin not shown in the drawings, which operates in a groove in the inside guide), hinges (I), and a screw-device (C), all made of brass. The air-gaps are 0.1 in. respectively. The arrangement is such that the armatures can be adjusted independently of each other in a 15° arc parallel to, concentric with, and of the same radius as the path of the inductors. This adjustment makes it possible to determine directly the phase-relationships between the two circuits.

The tachometer (F), with which frequency is measured, is an electrical instrument made by the Joseph Weidenhoff Corporation of Chicago.

The motor (G), around which this apparatus is built, is a Dumore, ½-H.P., 116-volt, universal motor, wound to give a maximum speed of 10000 R.P.M. under full load. It can be operated on either alternating or direct current.

The set-up includes a control-board on which are mounted two separate systems of potentiometers and tuning units, one for each of two independent circuits leading to a pair of Baldwin receiving phones. The tachometer and calibrating coils are also mounted on this board.

The alternator produces tones ranging in frequency from 125 cycles to 19000 cycles, depending on the speed of the motor and the number of teeth carried by the rotor. The speed of the motor can be adjusted by means of a system of rheostats, so that it is possible with the present set of disks to produce and maintain with a fair degree of consistency any tone within these limits. The tones as generated are relatively pure, and can be 'tuned' by grounding one side of the armature leads and inserting tone-filters in the circuits.

Phase can be controlled and measured directly by means of the screw devices, by which it is possible to adjust the armatures in relation to each other and the inductors.

Intensity can be controlled by adjusting the potentiometers in the two circuits.

The set-up is such that phase and intensity can be varied in binaural relation over a frequency-range extending from 125 cycles to 19000 cycles.

A NOISELESS EXPOSURE APPARATUS

By Miles A. Tinker, University of Minnesota

It is generally recognized that Dodge's form of tachistoscope[1] represents an ideal exposure apparatus for short times, because it provides a pre-exposure field and a post-exposure field with controlled illumination, and avoids the successive exposure of different parts of the field such as occurs when a window falls in front of the material for presentation. Dodge's apparatus has been adapted to many experiments, including the investigations of seen movement where two successive exposure fields must be shown, at the same optical distance from *S*, in addition to the pre-exposure field. However, Dodge's device requires in addition to itself

[1]R. Dodge, An improved exposure apparatus, *Psychol. Bull.*, 4, 1907, 10-13.

a timed control of illumination at two or more windows. For this purpose the falling shutter is usually too noisy; the rotating sector may be too rapid for control; electrical control of low-latency lamps may be vitiated by the lag of the lamps. The present device, for use with Dodge's tachistoscope, was devised at the University of Minnesota[2] as an accurate, noiseless and convenient control of the illumination in the case where only a pre-exposure and an exposure field are required.

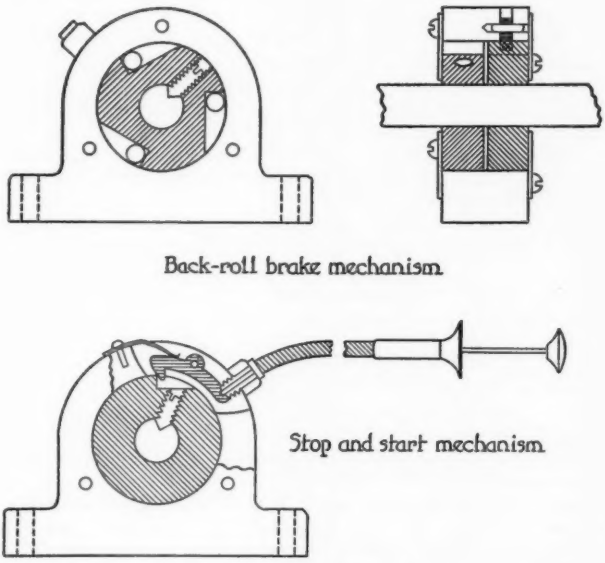


FIG. 9. BRAKE MECHANISM, AND STOP-AND-START MECHANISM, FOR NOISELESS TACHISTOCOPE

The tachistoscope is assembled on a frame of 1½-in. steam-pipe. This frame supports the Dodge tachistoscope and an illuminating unit opposite the windows. Between the illuminating unit (two lamps in separate boxes) and the windows is an aluminum disk with an adjustable sector which controls the time of exposure as the disk is made to rotate. The disk is 26 in. in diam. and 1/16 in. thick. The sector allows near the periphery of the disk an opening that can be varied from 0° to 55°. This opening permits light to enter the window that illuminates the material for exposure. The other window, which is used for the pre-exposure field, lies beyond the periphery of the disk, so that light from the proper lamp is ordinarily admitted to it. The variable sector, however, carries an extension be-

[2]The writer gratefully acknowledges the services of Mr. R. O. Dauphin of the University of Minnesota Shops, who designed the new parts for the apparatus.

yond the periphery of the disk, and this extension cuts off the light of the pre-exposure field exactly when light is admitted to the exposure field. The disk has attached to it a meter-stick that bears, on either side of the center of gravity, two weights. When the disk is released, it rotates under the momentum provided by the setting of these weights, in much the same way that Whipple's tachistoscope is operated.† Removable sectors of various sizes may be employed to increase the range of exposure interval.

The axle and disk have an automatic braking mechanism and a convenient stop and start mechanism. These are shown in Fig. 9. When set ready for exposure, the disk is held in position by a catch and is released by an ordinary shutter-release used on cameras (lower part of figure).

The disk, at the end of each swing, is prevented from rotating backward by the braking mechanism shown at the top of Fig. 9. Steel rolls, placed in tapering grooves, allow free forward movement of the axle carrying the disk, but immediately act as a braking device to backward rotation. This mechanism brings the disk to rest without noise or vibration provided the frame supporting the apparatus is sufficiently solid.

The exposure interval can be calibrated by attaching two cleats to the disk so that they cause electrical contact between two thin copper strips (platinum tipped) as the disk rotates. Either a kymograph and tuning-fork or a chronoscope can be used to record intervals. The writer, by the former method, has found a variability of less than 0.5 σ in successive trials.

APPARATUS NOTES

A Heat Grill

A simple heat grill may be constructed from two wooden plates with screws projecting up through them. The upper plate has the screws arranged like the black squares in a checkerboard with holes bored between where the red squares would be; the lower plate has the screws arranged to project through the holes of the upper piece, and of such length as to be flush with the ends of the screws projecting from it. One end of the upper piece may have phonograph needles inserted between the screws, with their points flush with the ends of the latter, so that a pain component may be added. This apparatus has certain advantages over previous heat grills.

Most of the grills described have tubes with circulating cold and warm water: Thunberg,[1] who seems to have made the first grill, used metal tubing coiled in flat spirals; Cutolo[2] used parallel systems of glass capillary tubes, requiring less water; Burnett and Dallenbach[3] kept the parallel arrangement but employed copper to obtain better conductivity and easier alignment; Twitmyer and Fernberger[4] used tubes coiled in a spiral around a core, to be grasped by hand, eliminating

†G. M. Whipple, *Manual of Mental and Physical Tests*, 1910, 227.

[1]T. Thunberg, *Upsala Läkarefors. Forhandl.*, 1, 1896, 489; *Mind*, 7, 1898, 143.

[2]F. Cutolo, this Journal, 29, 1918, 442-448.

[3]N. C. Burnett and K. M. Dallenbach, *ibid.*, 38, 1927, 418-431; 40, 1928, 484 f.

[4]E. B. Twitmyer and S. W. Fernberger, *ibid.*, 38, 1927, 119.

shoulders interfering with circulation; Bishop[6] used two systems of tubes, each supported from a capstan so that either could be presented separately or both together. Schlosberg and Carmichael[6] devised a simple, inexpensive grill, consisting of two comb-like elements whose teeth fit in between each other; since the temperature may be regulated by immersion in small pans of water, no large tanks of water are required. Attempts to demonstrate a fusion of temperature sensations with pain to give burning heat have been less frequent: Knight[7] stimulated simultaneously a warm and pain spot and obtained a sensation of "burning" which differed from that produced by an actual burn only in "extensity;" Ferrall and Dallenbach[8] used the grill described by Burnett and Dallenbach[9] with an alternating current to produce pain sensations; the fusion was not perfect, as the pain sensations reported were "too coarse."

The present grill is inexpensive and easy to construct, and it possesses the advantage of demonstrating 'burning heat' excellently by means of the addition of the pain component. To prevent rapid loss of heat from evaporation, the use of an electric hot plate is recommended instead of warm water; if water is used, merely the ends of the screws need to be immersed. Although intended for demonstration purposes rather than as a precision instrument, it has been possible with this apparatus to confirm Dallenbach's statement that the intensity of heat experienced increases with pressure.

Tufts College H. Barry, Jr.
W. A. Bousfield

An Apparatus for Measuring the Knee-Jerk

The variety of types of apparatus for eliciting the knee-jerk and measuring its amplitude indicate a general interest in applying laboratory technique to the study of this phenomenon. Rosenheim[1] constructed a hammer operated by a spring and attached directly to the leg. Lombard[2] employed a pendulum-like hammer with a device for setting and releasing, and a light rod carrying a steel needle which was attached to the foot and recorded the movement on smoked paper. Lee and Kleitman[3] used a stimulating device consisting of a spring hammer where the strength of the blow depended upon the degree of compression of a driving spring. For recording they used a fine wire fastened to the heel by means of a stirrup and passed over a pulley to a writing point, which recorded the movement on a kymograph drum. Foster[4] described a simple recording apparatus made up of a pivoted stick held by means of rubber bands against a support marked by a scale. The apparatus used by Tuttle[5] was relatively complicated. It consisted of a

[6]H. J. Bishop, *ibid.*, 38, 1927, 648 f.
[6]H. Schlosberg and L. Carmichael, *ibid.*, 43, 1931, 119.
[7]L. Knight, *ibid.*, 33, 1922, 587-590.
[8]S. C. Ferrall and K. M. Dallenbach, *ibid.*, 42, 1930, 72-82.
[9]Burnett and Dallenbach, *op. cit.*
[1]T. Rosenheim, *Arch. f. Psych. u. Nerv.*, 15, 1884, 184-201.
[2]W. P. Lombard, this Journal, 1, 1888, 5-71.
[3]M. A. M. Lee and N. Kleitman, *Amer. J. Physiol.*, 67, 1923, 141-151.
[4]W. S. Foster, *Experiments in Psychology*, 1923, 111-121.
[5]W. W. Tuttle, *Amer. J. Physiol.*, 68, 1924, 338-344.

pendulum hammer operated mechanically by a motor and a recording system with a stylus moved by wires and a lever against the tension of a spring. Johnson[15] described an electromagnetic hammer and a lever-system for recording the movement.

The apparatus to be described is adapted to class experiments where it is desirable to have several units of easily constructed equipment with a fair degree of accuracy. It possesses, moreover, the advantage of making possible the application of a blow of uniform intensity to an approximately constant area.

The hammer is made by weighting a squash racquet ball with lead shot, and suspending it from a stand so that the ball swings through an arc with a radius of about 1½ ft. and just touches the knee of the *S* when it hangs in a stationary position. In order to secure a uniform blow from the hammer the cord is attached to a square board, graduated like a protractor so that the height from which the ball is dropped may be determined. Lateral movement of the ball may be minimized by attaching a second string to the ball and fastening it to the stand so that it forms an angle of at least 30° with the original string as it hangs in a perpendicular position.

The recording device consists of a spring tape-measure of the type that allows the tape to be drawn from a metal case, and that contains a ratchet mechanism preventing the return of the tape until a button is pressed. The tape is fastened to a block of wood, heavy enough to remain stationary on the floor as the tape is drawn from the case. The case is attached to one of the surfaces of the block so that it is tilted about 30°. A piece of string is tied to the end of the tape and to the *S*'s shoe so that the measure is about 2½ ft. from *S*'s heel. Any movement of the foot should result in the pulling of a definite amount of tape from the case and the distance may be read directly. Within the range of the angular displacement of the foot in the average knee-jerk the error is practically negligible.

Tufts College H. Barry, Jr.
W. A. Bousfield

A New Commercial Stereoscope

The Perser Corporation, 450 Fourth Avenue, New York City, is now manufacturing stereoscopic pictures which give the appearance of depth to an observer who views them from many different points of view. In looking at the picture no special device is used in front of the observer's eyes. These pictures have been given the trade name of *Depthograph*. The fundamental principle of the depthograph was discovered by Dr. C. W. Kanolt. The following description of the apparatus is written by Dr. Kanolt and is reproduced here by his permission.

"Depthographs are made by a camera which moves in an arc around the object photographed, while the exposure of the photographic plate is being made. The plate-holder of the camera contains a lined screen which consists of a glass plate having on its rear surface an opaque black coating with parallel transparent lines in it. These lines are vertical and are very narrow. During the exposure the photographic plate is moved horizontally, the total distance it moves being about equal to the distance between two lines. At any instant the camera forms a photographic image only on those parts of the photographic plate that lie im-

[15] C. A. Johnson, *Amer. J. Physiol.*, 82, 1927, 75-83.

mediately back of the transparent lines. This image presents the aspect of the object photographed as seen from the particular position the camera occupied at the time.

"After the photographic plate has been developed a positive transparency is made from it by printing on a glass photographic plate. This picture, when viewed directly, has a blurred appearance, for the reason that it presents to view all the images produced on the plate in the various positions of the camera. But when a lined screen is placed in front of the picture and spaced slightly from it and the two are viewed by transmitted light, the lined screen permits only a single clear picture to be seen from each direction. Then as one walks past the picture he sees changing aspects of the objects in the pictures just as though he were passing the real objects. Moreover, since his two eyes see the picture from slightly different angles, they see slightly different views, as in looking at a real object. This gives the picture a real appearance of depth.

"This stereoscopic effect is very real and convincing. It seems as though one could reach his hand into the picture and grasp the objects in it. Without close inspection, one can not tell where the real surface of the picture is.

"The depthographs have come into use for window display advertising. For this purpose they are mounted in the front of a display case which contains an electric lamp. They have many other possible uses besides advertising.

"Depthographs can be produced in colors by hand coloring and by color photography. Practically all now are produced in colors.

"Depthographs will probably be of use for educational and scientific purposes, but none are for sale for these purposes at present. Several new developments in relation to depthographs are under way.

"Earlier experiments on the production of pictures, which, like the depthographs, require no viewing apparatus were made by A. Berthier (1896), John Jacobson (1899), Frederic E. Ives (1903), G. Belloni (1904), and E. Estanave (1907). Their pictures comprised only two views of the object photographed, and were stereoscopic from only a certain point of view."

Depthographs were invented several years ago by Dr. C. W. Kanolt, but have been fully developed by Dr. Kanolt and Mr. E. E. Draper only comparatively recently.

Brown University LEONARD CARMICHAEL

NOTES AND DISCUSSIONS

The Work of the Division of Anthropology and Psychology in the National Research Council

Requests from a number of psychologists for a statement of the offices and the accomplishments of our conjoint division with the anthropologists have induced the outgoing chairman to print here parts of his annual report for the year 1930-1931. While the report had to dwell primarily upon the late activities of the Division, the chairman had also in view a more general account of the sanctions and the aims of American anthropologists and psychologists in forming and sustaining this branch of the National Research Council. The first decade of its existence has passed. The present year seems therefore to be a favorable time to review and to set a value upon the activities of the Division.

A review of these activities may be conveniently brought under five general topics: divisional committees, funds administered by the Council, conferences and meetings, researches and investigations originating in, or proposed to, the Division; and the relation of the Division to its two constituent sciences, to scientific organizations, and to the Council. We proceed in order under these headings.

(*1*) *The Divisional Committees.* When a problem of research involving several men or agencies or calling for preliminary discussion and planning is laid before the Division, you generally appoint a committee and charge it with a specific task. We now have anthropological committees working upon state archaeological surveys, accurate publicity, a special census of American Indians, and racial problems; and we have psychological committees for deafness, child development, national intelligence tests, psychology of the highway, and Russian translations.

The energetic and intelligent activities of the Committee on State Archaeological Surveys have resulted in several important publications, in the help and encouragement of many local archaeologists, in the coördination of archaeological agencies, and in the personal inspection and visitation by the Chairman (Dr. Guthe) of field operations, museum collections, and archaeological staffs. As a result of the industry of this Committee and of the Committee on Accurate Publicity, Minute Men have coöperated with *Science Service* in inspecting alleged discoveries, determining the authenticity of new materials, and giving general and discriminating publicity to reported fossils and artifacts. The Joint Committee on Racial Problems, established by the National Research Council and the Social Science Research Council, discussed at a conference in Detroit the possibilities of undertaking a coöperative study of black and white children in order to determine the origin of certain racial differences. As further joint action upon this matter appeared not to be feasible at the moment, your own Division has recently appointed a similar committee, which will, as we hope, seriously study this difficult matter next year and bring to us its recommendations.

On the psychological side, the large accomplishments of the Committee on Auditory Deficiency have reached the difficult stage of financing. You will remember that the two conferences on Hearing and Deafness attained the co-

operation of all the sciences involved in this field with the State, medical, and private agencies engaged in the care, education and relief of the deaf, and the prevention of deafness and hypocusia. It also formulated the most reasoned and complete program of research ever devised in this field. The persistent and competent efforts of the Chairman, Dr. Dunlap, to inform and interest foundations and other possible donors, together with an appeal from the Divisional Office to many individuals of generous habits, has not, until now, led to a definite promise of financial aid. This is a large and important undertaking in which every member of the Division may take a responsible and useful part.

Scientific interchange and advice have been continued by our Committee on Russian translations in Psychology. Although no translation either way has yet come from this interchange, it is of importance that Americans and Russians should be critically advised upon our current literature and especially that worthless and unsound books should be prevented from passing current in either direction.

Your committee on Child Development and its subcommittee on the Selection of Abstracts have been active and effective. The *Child Development Abstracts* have appeared regularly and in good form, and the demand for them has continued. Letters addressed to men and women engaged in the scientific study of children have made it appear that a thoroughly revised and extended edition of the Directory of Research in Child Development is needed and that subscriptions will repay a large part of the cost of editing and publishing. The work is now well in hand and the new directory will presently appear. When the full report of the Committee comes before you, you will observe our relations to the recent White House Conference on Child Health and Welfare, and you will be asked to decide upon the best way to make this Committee useful in the future. As the activities in National Intelligence Tests chiefly relate to the year's conferences, they will be considered with that topic.

The history of the Committee on the Psychology of the Highway well illustrates the advancement of research through a well-considered plan which enlists the support of outside agencies. Those of you who attended last year's Annual Meeting will recall the large amount of substantial research reported from Ohio State University by the Chairman, the late Dr. Weiss. Never well and always threatened by complete disability, Dr. Weiss sustained, along with his regular University duties, the work of a large group of competent associates devoted to our special task. When compelled to resign, he had nearly completed an extensive report of the psychological factors in motoring. This report is soon to be submitted and published by Ohio State University.[1] In succeeding to the Chairmanship, Dr. Dunlap found it expedient to transfer the chief field of work from Columbus to Iowa State College where it is vigorously carried on (as you will presently hear) by Dr. Lauer, who had conducted several of the most important researches of the Committee while he was resident with Dr. Weiss at Columbus. The encouraging state of the researches will be told you by Dr. Dunlap who has just now returned from Ames.

[1]The volume appeared in June. A. P. Weiss and A. R. Lauer, *Psychological Principles in Automotive Driving*, Ohio State University, 1931. Pp. vii, 165.

(2) *The Division's Part in the Council's Administration of Funds.* The chief funds entrusted to the Council for specific expenditure are two; fellowship funds and grants made in aid of individual researches.

(a) *The Post-Doctoral Fellowships.* The Council receives relatively large cash grants from the Rockefeller Foundation for the support of fellows in America and in foreign lands. The selection, appointment, travel, and support of these fellows are matters delegated to three fellowship boards; one for the Physical Sciences, one for the Medical Sciences, and one for the Biological Sciences, including Anthropology and Psychology. The first of these boards, the oldest, has alone distributed more than a million dollars to fellows in Physics, Chemistry and Mathematics. The boards are creations of the Council and are self-perpetuating by gradual replacement. Each division concerned is represented by its Chairman and by other members which you and other like bodies nominate to the Council year by year. At least two psychologists scrutinize the psychological applications and two anthropologists the anthropological. So far as is possible each applicant is personally interviewed (preferably in his own laboratory) by a board-member competent to pass judgment upon the problem proposed for research under the fellowship. Former publications are read, local supporters seen and written to, and the candidates' abilities and promise estimated. In case of appointment, the board undertakes to discover the place, here or abroad, best fitted to the scientific and cultural needs of the individual. Not infrequently the final choice of place is left to the Chairman of the Division. It may be made only after several weeks of correspondence, travel, and the soliciting of advice of those men best fitted to counsel upon the particular research proposed. The fellow is assigned only upon the pledge of a sponsor, who agrees to provide space, equipment, materials and oversight. By this means, the fellow becomes a member of a resident group of researchers, and his adopted department, university or institute makes a substantial addition to the support of his research. At this moment our own Biological Board has before it more than 100 applications to be considered at its spring meeting next week. Although the primary intent of the award is to select, encourage, mature, and qualify good men past the doctorate who are permanently devoted to their sciences and who will presumably make substantial contributions, the year or more of research must be carefully planned and placed under good auspices. The task of selecting fellows is difficult. When taken seriously, it is not a job to covet. On your side, you should—provided you are to continue your support of the fellowships—select your representative with care, provide well-trained doctors from your own laboratories, encourage these to apply, convey exact information on conditions, and generally sustain your fellowship board. You may, of course, raise the question of the absolute or the relative value of the post-doctoral fellowship. During the last year this question has been seriously raised by this and other divisions. It has been debated by the fellowship boards. It has been pertinently raised by the Foundation, which now approaches the term of its present grants and asks to be assured of the high value of the fellowships. Opinions are abundant. Most of them are generally favorable to a continuance of the grants. But more than general opinion is needed. The problem needs research. Are the best men appointed? Do men apply who have nothing else to do? Are fellows better off professionally than they would have been had they taken positions immediately after the doctorate or had migrated to business or to other

professions? Would they have acquired greater competence in their professions by passing directly into positions of teaching and research? Do fellows well provided for a year or more grow lazy? Or superior to teaching? How many are distracted in a new setting and thus waste time? Is the market for them saturated? Are there too many fellowships?

The Division has now had about 70 annual occupants of these fellowships from Psychology alone at a cost of about $150,000. The figures for Anthropology are still incomplete. It seems to be time to make an objective review and to attempt considered answers to some of the questions just now raised. The first small beginning appears in the list of doctors graduated in Psychology during the decade 1920-30, which has been sent to about 125 American psychologists active in research and in their science generally throughout that time. These men have been asked to make a simple assortment, in three classes, of as many of these men and women as they know. Practically all of our fellows are included in this list. A gross comparison of the entire generation of young psychologists with the smaller group of fellows will thus be begun. Further facts we hope to get by interrogating the fellows, their sponsors, the chairmen of their doctoral committees, their scientific mates, and others. The problem is very difficult; but we hope that it is not wholly insoluble.

(*b*) *Specific Grants to Researches in Course.* Last year the Rockefeller Foundation placed $100,000 at the disposal of the Council to be used as Grants-in-Aid. Most of this sum has been expended to help researches actually in process, which were also supported in part from other sources. Most of the grants were for a few hundred dollars and very few exceeded one thousand dollars. Anthropology and Psychology have, as I think, had their just share. The general impression prevails that real problems and competent men have been greatly aided by these modest sums. In several cases, the Division has been able to give information or to render assistance that has advanced the research or reduced its cost. One such instance relates to the securing of special prices for films to record the speech of native cultures, and another to the search, from Pasadena to Berlin, for the best means of recording bird-songs in a study of inheritance and imitation, to the saving of several hundred dollars in the price of apparatus, and in securing the coöperation of a physicist and a psychologist.

More funds are in hand and the Committee on Grants for Research is soon to hold its final meeting of the year. The simple conditions under which these awards are made can easily be obtained from the office of the Committee. The divisions of the Council are represented upon this committee in order that the requests may receive competent and discriminating attention.

(*3*) *Conferences and Meetings.* My first and elementary notion of a conference at the National Research Council was of a group of men who reluctantly left their own individual researches and congregated in Washington that they might incubate some vague project involving group-research and group-discovery. While this notion caricatured the actual conferences which I subsequently entered, I am led to infer, from opinions often expressed by psychological and anthropological colleagues, that it is here and there entertained today. One common sanction for this notion recites the artistic character of the real man of science, who must retire from his fellows and from worldly distractions in order to create.

"Einstein plays his violin," concludes a recent rhapsody written in this temper. "I have not much faith in coördinating research activities," writes a psychologist. "I believe that scientific work, not dissimilar to that of the artist, is so much a matter of individual initiative that organized coöperation will not be favorable to its development." Now it is commonly known that men can waste time by conferring upon scientific and other subjects; but the advocates of the isolated cell for the 'inspired creator of truth and beauty' sometimes overlook comparable losses of time and energy in individual research uncombed by group-wisdom, in verbose and muddled report and publication from the isolated investigator, and in the neglect of competent opinion which may restore balance and perspective to the encapsuled man of learning. Without trying to mar the image of the great man creating and producing in retirement—an idol set up in this country nearly a century ago when our writers and historians were first impressed by Continental universities and scholars—we may with propriety observe the benefits to pure science—as well as to beautiful letters and to fine arts—of certain forms of socialization and of certain modes of coöperation.

One form of socialization and of coöperation is the conference; but within the sciences alone the conference is of several kinds, each kind possessing a distinctive function and acquiring its proper value by its scientific accomplishments. The main varieties of our own conferences of the past decade may be easily distinguished by historical inspection.

The first is the conference on *concepts*. An example is last year's Conference on Regional Phenomena, where representatives of Anthropology, Geography, History, Biology, Economics, Sociology, and other disciplines met to bring about (in the words of Chairman Cole) "a correlation of the various ideas of regions" and to "extend or reshape the concept of regional study." The chief intent of such a conference would seem to be the clarification of useful but ambiguous terms, mutual understanding between men researching in adjacent and overlapping fields, and the comprehension of plural contexts which cluster about common terms variously employed.

A second kind of conference *throws into commission large problems imperfectly formulated* for research. Of such a kind were the conferences organized on Hearing and Deafness by Chairman Dunlap. An essential part of the methodology here was the functional union of the sciences which research on hearing with agencies of education, care, and cure. The benefits of such a union were sought in the discovery of our present knowledge and ignorance of the total topic. An exhaustive and orderly plan of research issued. Only adequate funds are now wanting to inaugurate under various auspices and agencies a decade of scientific research upon real problems never before thrown into commission and set into a just perspective. Another instance is afforded by the Council's conference on Problems of Human Migration. This type of conference easily goes wrong when the problems are vague and artificially chosen, or when they are not accessible to research of a scientific kind.

A third type of conference, related to the second variety, *unites sciences already researching upon a common theme*. An early series of conferences upon Vestibular Research, a legacy of the Great War, exemplifies it. Out of this series came a number of individual researches which gained greatly in point and significance because physiologist, embryologist, otologist, and psychologist investigated and

wrote in the light of a common knowledge, where before misunderstanding, rivalry and dislike were the rule. The Council's work came to a natural end; but current research is made possible by it and by the collection and wise use of funds which it suggested.

Still another kind of conference in our annals *collects and digests recent materials*, produced by individuals and groups, and makes it accessible to many. Here we may place the three great conferences on Child Development, organized by the Division and its special committee. Each conference offered an occasion for the exposition and criticism of recent studies, and it furthermore raised the standard of research in the fields touched by the valuation of materials good and bad. Such conferences supplement, in a useful way, the periodicals, the laboratories, and the institutes devoted to the scientific study of children.

Again we have had ambitious conferences which undertook the herculean task *of bringing order into large fields* which variety of interest and of method had *left chaotic*. Such a field in Psychology is that of Individual Differences, a complex topic which suffers until descriptive classification and quantitative valuation bring order. To such an undertaking psychological objects offer peculiar difficulties. A first general conference brought together several points of view and many methodologies. It also produced selected bibliographies and a rapprochement of rival schools. Smaller conferences (two this year) are now uniting the most capable men and women for reflective consideration of such special matters as Individual Differences in Growth and Development and in General and Special Abilities.

A sixth occasion for conferring is the *discovery of a baffling problem which stands in need of scientific investigation*. Such a problem is the origin of the psychological and social differences obtaining between blacks and whites, or between whites and Indians. Last spring's Detroit Conference on Racial Differences, *e.g.* discussed the question whether a possible and sound method of observing young children from two racial groups under controlled conditions could be found. The problem is real. Contributions to it have been made by the cell-method. But cell-contributors also contributed to the Conference, and it seemed to be generally agreed that the Detroit meeting helped toward a clear statement of the problem and of the difficulties and pitfalls of old procedures. By defining a problem and by elevating the standards of procedure under it, a conference may pay its way. If you are aware of the funds wasted in a decade by incompetent or inconsiderate research, whether in the cell or in the group, you will apprehend one important service to be performed by the conferring of wise and critical men upon such tangled topics.

The last variety of conference with which I shall tax your attention is the variety which *seeks extra-scientific aid for research*. Our conference held in Chicago a fortnight ago on Pleistocene Man exemplifies it. Geologists, anthropologists and archaeologists, noting the scanty and imperfect evidences of Man in America during the ice stages, asked the Council to invite to conference representatives of the railways and the engineering societies, and of the larger organizations controlling the excavation of stone, sand, gravel and earth. The membership of this Conference included men from the Smithsonian, U. S. and State Geological Surveys, U. S. Army Engineers, Carnegie Institution of Washington, American Museum of Natural History, Peabody Museum of Yale, Phillips Academy

Museum, Field Museum, Museum of Anthropology at Ann Arbor, presidents and other general officers of most of the great railways of the Midwest, the National Sand and Gravel, National Crushed Stone and American Roadbuilders Associations, and Associated General Contractors. The most suitable geologists and archaeologists laid the problem before the conference and proposed a definite method of coöperation between the sciences and the other agencies involved. The debt to excavators for increasing our present knowledge of primitive man in Europe, Asia and Africa was made clear. Practically all the agencies sought have pledged systematic aid, railroads volunteering to inform their entire personnel and national excavating organizations proposing to acquaint through their publications thousands of local affiliates. Geologists and anthropologists are composing lists of men who will promptly visit a locality whence a discovery is reported. This Division has offered to serve as a clearing house. Science Press, Associated Press, and other news agencies have already given wide publicity to the conferences and its plans, and they promise their services of report and communication. As the primary necessity is the geological dating of skeletal and cultural objects found, the immediate presence of geologists and paleontologists is of the first importance.

Thus our conferences have clarified concepts, thrown large projects into commission, united contributory sciences, collected and digested materials, ordered chaotic fields of investigation, discovered the locale of baffling enigmas, and sought extra-scientific aid for anthropological and psychological problems. It appears, then, that the divisional conferences may—with more or less success or failure—be critical, organizing, uniting, assorting, ordering, discovering and aid-seeking. These are typical conferences. They should have been more widely illustrated from the entire history of the Council. The Chairman has been limited by his information, choosing examples which he best knew. You may care presently to cite and to discuss other conferences and their accomplishments. But enough instances have been described and distinguished in type to make it clear that conferences of the National Research Council are not competently described and judged as table-groups where men meet, to the neglect of their own individual researches, to inquire vaguely what they can conjointly do to advance their science.

Some of our members are skeptical of all conferences when used as scientific instruments. One of them suggested not long ago that we call a conference to destroy all conferences. Men are not always specific in their objections. It is common to cite one or two conferences which seem to the individual unsuccessful or to engender differences of opinion. In some cases, the member judges by his personal experiences and is quite ignorant of actual accomplishments; *e.g.* a member alleges that he has not systematically used the "Suggestions to Writers" which sprang from our conference of editors and publishers, overlooking the constant request of young writers in Anthropology and Psychology for the pamphlet and its obvious influence upon literary and scientific standards.

No scientific method is above misuse or abuse. A judicious survey of our history might reveal conferences utterly useless and others actually harmful; but valuation should be both historical and comparative, not selective and emotional. A search of our anthropological and psychological journals for the decade would turn up records of individual and unsocialized researches which had not either advanced our two sciences. Trained men and real problems can probably

be trusted to devise suitable methods and to make the best use of the laboratory, the calculating machine, and the conference-table. Men who insist upon rivalry among these resources might well consider whether these various means are not rather supplementary than antithetical.

(4) *Researches and Investigations Originating in, or Proposed to, the Division.* During the course of every year a good many matters regarding research or regarding the general welfare of our sciences come before the Chairman for action. Some of these matters originate outside and some inside the Division. Taken all together they seem to offer one of the major sanctions for our existence. As these things are less likely to come through general channels to American anthropologists and psychologists, I shall give you samples and instances from the current year. Some of them will later be presented to you for action.

You all know of the Tolman-Tryon research upon functional inheritance in the rat. The psychological laboratory at Berkeley has succeeded in segregating from a heterogeneous stock two f_4 groups of 'bright' and 'dull' individuals, widely distinguished in accomplishment in the maze. Functional aptitude evidently depends here in part upon stock. It is important that the research, which appeared to exceed local resources, should go on. Dr. Tryon's term as Fellow under our Board had expired. Through the Chairman of the Division, Dr. Tolman's budget for a five-year period was brought to the attention of foundations and individual donors. The Division secured $1000 from the Committee on Grants-in-Aid. At the eleventh hour, when larger funds had been virtually promised, the University of California was induced to appoint Dr. Tryon to a permanent staff-position.

The inventor of a successful device for continuous film-projection asked the Division to provide a comparative research upon strain, fatigue, and clear perception under intermittent and steady illumination. The Chairman visited the inventor and inspected the method. A Committee of the Society of Motion Picture Engineers has now asked its Board of Governors to provide fellowships for this research, and this Division is arranging for the work in one or more psychological laboratories in our universities.

At the request of the National Committee for Study of Social Values in Motion Pictures, your Executive Committee appointed the present chairman to serve on the Board of that organization. The director of the organization has sought the advice of the Division upon its various current researches and has asked the Chairman to attend its annual Research Conference at Columbus in May and the New York meeting of the National Committee in June. Its researches are chiefly conducted by psychologists in several of our larger laboratories.

Dr. Boas has recently brought to us a comprehensive plan for long-term research upon the integrity of the life-cycle. The chairman is in conference with foundations and various individuals upon the organization and the financing of the research. The plan proposed would insure the support through a term of years of much of the anthropological and anthropometric life-work of Professor Boas and his collaborators. One activity of the agency would be the periodical examination of selected individuals of various races over a long period of time in order to discover the temporal integration of the organism and its racial and environmental conditions.

The Chairman has made a preliminary study of the needs, possibilities and agencies for aiding and encouraging research, particularly in our two subjects, in the South. A good deal of material has been collected. It includes the report of Dr. Seashore to the Council after he visited and inspected a large number of southern universities and colleges in 1923-24, and also a voluminous report on Chemistry and Chemical Industry in the South. Conversations with general officers of the Council led the Chairman to believe that when fundamental education in the Southern States is sufficiently advanced to give a basis to research in our subjects, the Council will be friendly to proposals designed to advance serious investigation in that region. A request from Dr. Moorehead for a small fund to help finance a lecture tour on archaeological subjects in the Southeast was approved and taken to the Council, which decided that the funds would be more appropriately raised by the local institutions visited.

In December an inquiry was addressed to leading anthropologists throughout the country, inviting them to submit to the Division suggestions and proposals for research, which, in their opinion, the Council might find means for advancing. The Chairman noted the danger in a dual division of the Council of according to the Chairman's own subject undue consideration during a given year. The following suggestions were received:

(1) The formation of a committee on the use of the airplane in scientific research. This committee should include representatives of archaeology, geography, forestry, and biology, as well as technical 'air people.'

(2) The discovery and use of a means for placing the proper emphasis upon practical values of physical anthropology. It is pointed out that the theoretical study of anthropology has sometimes overlooked the value of its applications to everyday life. Examples of such applications are given in the study of human growth with its manifold expression at different levels throughout life (establishment of race, age, medical misadventures, mental development, social relationships). Special training of students in these practical applications is also suggested.

(3) Tabulation and classification of artifacts made and used by the American Indians. This material is at present uncorrelated, due largely to its being scattered over the country in the various museums.

(4) Encouragement of coöperative research among physical anthropologists on skeletal material already gathered in the Eastern United States.

(5) The encouragement and creation of interest in archaeology among various organizations in the southern states. It is suggested that this could best be accomplished by personal vists from an eminent archaeologist. A carefully planned trip during six or eight weeks early in the spring could include conferences and speeches in the most important cities and institutions.

(6) Conference among archaeologists, geologists and commercial excavators on the preservation of Pleistocene remains in the Middle West.

(7) Permanent record of Indian languages by means of sound films and orthophonic records.

Upon receipt these suggestions were collected and sent again to anthropologists with the following note.

"Will you comment upon the value and feasibility of these suggestions, and give concrete advice on the conduct of work based upon them? If other anthropological projects, which might be instigated or encouraged by the Division,

have occurred to you, or are suggested by those noted in this memorandum, the office will be glad to hear from you at your earliest convenience."

With the replies to this second letter, there came from Dr. Todd carefully worked out plans for researches on the following subjects:

(1) The growth of personality

(2) The effect of hormones upon reproduction and general constitution

(3) The manifestations of healthy functional activity in the reproductive system and

(4) Anthropoid growth.

The proposals made and a part of the voluminous correspondence resulting therefrom were sent to Vice-Chairman Lowie. It is fitting that Dr. Lowie should, either at this meeting or at a future time, bring the material before the anthropological members of the Division in any form that he may select.

The Chairmen of Medical Sciences and of this Division have under consideration a plan for improving the scientific basis of the study, classification, and treatment of psychological disorders (or 'mental diseases' as they are more frequently called). The Chairmen have thought that an unique opportunity was now presented to America. In the understanding and the relief of these disorders, speculation, unattested theory, and the tenets of competing schools have played a primary rôle. The scientific foundations are uncertain. The Chairmen have accordingly proposed the establishment of a small Commission, to be composed of a member from each of the following subjects; Physiology, Cytology, Chemistry, Pharmacology, Genetics, Anthropology, Psychology, Pathology, and possibly one or two others. The members of the commission will preferably sit for a year studying their common problem from various points of view, visiting institutions and calling before it doctors, psychiatrists and representatives of societies for mental hygiene. Possibly it will also distribute funds in aid of researches contributory to the object in view. It is hoped that the report of such a commission, after intensive study, may place the general subject upon more secure foundations in the sciences, and advance the study and treatment of the psychologically disordered. The proposal has been placed before one of the donor-foundations, which is giving it careful scrutiny.

(*a*) *Neural and Cerebral Radiation.* At the request of Chairman Curtis of the Division of Biology and Agriculture, your Chairman placed before the Committee on the Effects of Radiation on Living Organisms an outline of research on the application of X-rays and other forms of radiation to nerve and brain. This research has been undertaken conjointly by the Departments of Psychology and Physiology at Cornell University. This part of the total plan of Dr. Curtis' committee has been laid before one of the donor-foundations which has asked your Chairman to visit the Foundation next week and to report more fully with an estimate of probable costs over a term of years.

(*b*) *Growth and Development of Indian Children.* Dr. Wissler and Dr. Aberle have brought to the Division a plan for securing and interpreting anthropometric and functional data from Indian children in our Southwest. The Chairman has secured the coöperation of the Smithsonian Institution and the Bureau of Indian Affairs in the Department of the Interior. Plans for coöperating with the Federal Government and for securing financial assistance are well under way. The work

is carefully planned and a budget predicts its approximate cost over a term of years. Secretary Wilbur of the Department of the Interior has endorsed the study.

Incidental duties of the Chairman include participation in the Annual Meetings of the American Psychological Association at Iowa City, delivery of the vice-presidential address of Section I, A.A.A.S., chairmanship of the committee of award of the thousand-dollar prize of the A.A.A.S., meeting of the Research Board of the Otological Society of America in New York, the granting of fellowships and other awards for otological research, the presentation of divisional problems to the spring meeting of the Experimental Psychologists at Poughkeepsie, and conferences at the request of the American Psychological Association with chiefs of classification in the Library of Congress and the Dewey Decimal System, looking toward a fundamental revision of the psychological rubrics of our great libraries. Considerable travel has been involved in interviewing applicants for fellowships, securing apparatus and equipment for grants, attending meetings, and conferring with foundations and other donors.

Innumerable inquiries respecting topics of research, publications, men and institutions come to the Chairman's desk by letter and call during the year. Taken all together, this incidental service, though inconspicuous, is generally regarded within the office as of first-rate importance to our dual division.

Many conversations with the chairmen of Biology and the Medical Sciences upon common researches and upon the general conduct and welfare of the Council have been informing and useful. These three divisions form a natural larger unit which has supplied a scientific interchange for which the general organization of the Council makes very little provision. They have taken the position that, since they represent within the Council the sciences of life, it is appropriate for them to recommend to the Executive Board and General Officers of the Council such modifications of procedure and changes of organization as promise to improve the conditions and the instruments of research within these sciences in the country at large. Close relations with the Division of Physical Sciences (Dr. Richtmyer, Chairman), especially upon the subject of the fellowships, have likewise extended the interests and enlarged the influence of our own Division.

(*5*) *Relation of the Division to its two Constituent Sciences, to Scientific Organizations, and to the Council.* In establishing this Division, the Council hoped that it could well serve our two sciences. Many have agreed that it has; but here and there has appeared skepticism. Some men have felt superior, in a professional way, to a war-product, organized outside their borders, endowed with money accumulated in business, and operated by men in whom they had little interest. Distrust, misapprehension, and the loud voice of the 'scientific conscience' led to formulas and phrases which were designed to extol the individual investigator and to condemn all corporate means and methods. The phrases persist. "You can't serve science that way." "The man of science must work by himself." "The Council does nothing but legislate and discuss." "Committees and conferences are useful only in business and in society." "If the Council were of any use, it would give me money for my valuable investigations." "All chairmen become hard and dictatorial, losing their scientific honesty." You know the stock phrases! Some of you have heard them repeated as with the unction of a first recital.

Probably the first means to a true estimate of our Division is by way of its history. We have finished a decade. That term goes beyond the limits of reliable memory. We greatly need an objective history. Our industrious secretary, Mrs. Britten, has made a good beginning by her carded record of about five hundred entries, well sorted and indexed. That is of inestimable value to the Chairman, who does well to put it to constant use. Many unconsidered criticisms of the Division can be dissipated by reference to its facts. The present Chairman cheerfully confesses that it has frequently straightened his biased judgment and confused his assured ignorance.

As another means of interpreting our first decade, we have this year addressed to the ten past-chairmen a request for their individual opinions upon the objects, methods, shortcomings and accomplishments of the Division. All but one chairman responded. You will have these questions and answers placed in your hands. At present I shall comment very briefly upon them. As you might suspect, you will find various degrees of care and wisdom exercised in the presentation of the replies, which may well suggest a belated test for the vocational choice of divisional chairmen.

(*a*) *Functions.* All Chairmen agree that the Division has been useful; though they differ upon its most important functions and accomplishments. You will find here thoughtful suggestions toward important changes in our second decade.

(*b*) & (*c*) *Organization.* Most approve the present arrangement; though some find our dual organization awkward, especially for Anthropology, and some deplore the present lack of preparation of the chairman for his duties.

(*d*) *Committees.* The Chairmen find committees good, bad and indifferent; but they agree that this agency is necessary. They say that its usefulness depends largely upon the chairman and upon his choices.

(*e*) *Conferences.* All approve conferences; but most find certain types of conference either useless or evil.

(*f*) *Division and Council; Funds and Fellowships.* General satisfaction is expressed upon the relations of the Division to the Council; though the virtual limitation of these relations to business matters and routine is deplored. Fellowships and grants are endorsed and the opinion is expressed that the Council itself rather than the divisional chairmen should take the chief initiative in securing outside funds for research.

(*g*) *Division and Scientific Associations.* The chairmen agree that the functions of the Division could not be transferred to the anthropological and psychological associations. They suggest, however, a closer interaction. Especially should the associations revise their methods of choosing their representatives upon the Council, making sure of men who are both competent and willing to work.

(*h*) *Use of Past Chairmen.* A more extensive use of past chairmen is urged; but various difficulties (expense, numbers, travel, etc.) are noted.

(*i*) *Support.* All agree that our two sciences should support the Division. In the words of one chairman:

"It should be made clear to anthropologists and psychologists that Anthropology and Psychology will be benefited by the Division in exact proportion to the

amount of moral support and helpful effort which they are willing to put into it. I feel very strongly that while the Division has not accomplished miracles, it has done very well indeed."

(*j*) *Funds for Research.* As all members of the Division know, the Council provides salaries for chairman and secretary, a modest sum for general maintenance and operation, a mimeographing and multigraphing service, and other minor facilities. But an entire decade has not been sufficient to remove, in some quarters, the impression that the endowment or other resource of the Council does, or is designed to, produce a large income for the support of scientific researches. Men of science do not always extend their critical canons beyond their own professional interests. Income from the Council's investments has cared for maintenance and operation; although income will have to be increased or operating costs sharply diminished after the next fiscal biennium. The Council administers many outside funds designed for specific purposes, and it has received from the foundations and other donors large sums for the support of fellowships, and more modest sums, of late, to be devoted chiefly to small grants for established researches actually in process.

When the Division has needed other funds it has been expected to solicit them outside the Council. This duty now falls chiefly upon the Chairman of each division. All the divisions taken together have in the last decade raised a large total sum in this way. It is frequently remarked by anthropologists and psychologists that, 'of course,' our Division has miserably failed in this direction. In view of this common remark it is worth observing that the Division has within its first decade found over a half-million dollars for research. If we add the fellowships financed in Anthropology and Psychology and the smaller grants for specific aids, the sum approximates three-quarters of a million. Should we take into account economies effected by the Division in research done for universities and sums extracted from universities for research within their walls, our total might well stand near a million. This is not a large sum when it is compared with the total expenditures of the major foundations; but it is a sum not to be despised and not to be overlooked.

Throughout this report the present chairman has emphasized the relations of the Council to American anthropologists and psychologists. These relations seem to him to be of great importance. Where these men refuse to be responsible for their Division, the Divison fails. An emotional conflict with the Council here and there among our people has done harm. A fear of machinery, a cold view of money unless it flows one's way, dislike or envy of colleagues, a conviction of superiority to outside agencies, distrust of conjoint performance, introverted tastes, bad judgment within the Division, unjust discrimination, the failure of projects, are some of the elements in this conflict. Simple objectivity and improvement under experience would seem to be sufficient to remove all of them.

The Division is neither a rich patron to the sciences nor a worker of miracles. Men who know its history believe, however, that it has usefully served us and that it has supplemented our other means and agencies of research. It can be greatly improved. Improvement rests with the anthropologists and the psychologists of America.

Cornell University MADISON BENTLEY

A Neglected Possibiltiy in Frequency Theories of Hearing

The striking experiment recently performed by Wever and Bray[1] seems, at first glance, to provide a final answer to the long disputed question concerning the nature of the neural response to acoustic stimuli. Wever and Bray found that the action currents led off from the functioning acoustic nerve produced a telephone tone of the same pitch as the sound-stimulus producing the nerve-currents. A natural interpretation of this result is that the successive nerve-impulses are produced at a frequency equal to that of the sound-stimulus. We wish to point out, however, that the results of Wever and Bray are not unambiguous and that, for the determination of the precise nature of the response in the acoustic nerve, a final experimental step remains to be taken. The experiment of Wever and Bray establishes the fact that frequency-characteristics like those of the stimuli are possessed by the auditory nerve-currents, but not that the individual nerve-impulses themselves follow each other at these frequencies. It is possible that their telephone detector was responding, not to the frequency of successive nerve-impulses, but to *groups* of such impulses, and that it was the groups which were being produced at the same frequency as the stimulus. If the individual nerve-impulses occurred at frequencies greater than the frequency range of the telephone membrane, the membrane could not vibrate in unison with these impulses so as to indicate their presence by a tone of specific pitch. It could, however, vibrate in unison with groups of such impulses, if the groups were occurring at frequencies within the frequency limits of the membrane.

What is here suggested as possible is that in the response of the acoustic nerve a situation exists similar to that obtaining in the transmission of sound-messages by radio-waves. There, as we know, the electromagnetic waves radiated are usually of much higher than acoustic frequencies—ranging between limits of about 10,000 and 300,000,000 cycles per sec. These high frequencies are, nevertheless, capable of transmitting sound-messages by being broken up into groups which reoccur at acoustic frequencies. The frequency response of the telephone or loud-speaker corresponds to the frequency of the groups of electromagnetic waves and not to that of the waves themselves.

An answer to the question of whether the frequencies detected by Wever and Bray in their telephone were the frequencies of successive nerve-impulses, or of groups of these impulses requires one more experimental step, that of substituting for the telephone an instrument, such as a high frequency oscillograph,[2] that is capable of indicating all the details of the action currents and not merely their integrated effect.

We are not concerned here in elaborating a theory of audition, but merely wish to point out the need of this final step for establishing the exact nature of the acoustic nerve response. It may be in point, however, to see whether certain of

[1] E. G. Wever and C. W. Bray, The nature of the acoustic response, *J. Exper. Psychol.*, 13, 1930, 373-387.

[2] Two instruments, in particular, suggest themselves as capable of the high frequency performance required here, the loop (*Schleifen*) oscillograph, as developed by Rosenberg, and the cathode ray oscillograph, as developed by Gasser and his co-workers. For a discussion of recent advances in the use of high frequency instruments in the recording of action currents, see H. Rosenberg, Untersuchungen über Nervenaktionsströme, I. Methodik: Röhrenverstärker und Schleifenoszillograph, *Arch. f. d. ges. Physiol.*, 223, 1930, 120-131.

the facts of neural function and of audition do not establish a certain plausibility in favor of the possibility raised above and to that extent make the experimental step suggested all the more necessary.

The suggestion that the units in the nerve-response which are reoccurring at audio-frequencies are possibly groups composed of impulses occurring at still greater frequencies furnishes an acceptable neural correlate for intensity-steps in auditory experience. It may be supposed that the frequency of the impulses, which make up a given group, varies with the *intensity* of the stimulus, just as is true in the responses of other sensory nerves, as shown by the experiments of Adrian and his colleagues.[3] Frequency relations in the neural response may serve

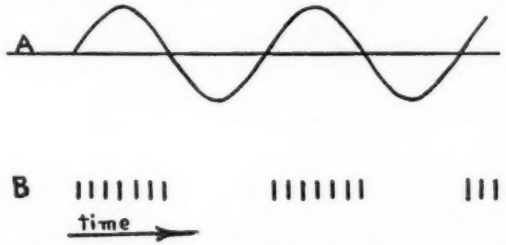


FIG. 1. ILLUSTRATING CORRESPONDENCE BETWEEN STIMULATING SOUND-WAVE AND CORRESPONDING NEURAL RESPONSE

A represents the sound wave, and B the successive nerve-impulses. The short vertical lines in B represent the successive nerve-impulses, the frequency of which depends on the intensity of the stimulus. The frequency of the groups of impulses corresponds to the frequency of the sound-wave.

as correlate *both of intensity of stimulus and of frequency of the stimulating sound-wave*, because it is possible that there are frequencies of *two orders of magnitude* in the neural currents, that of successive impulses and that of the groups. The nerve-current may be thought of as so synchronized with the impinging sound-wave that the succession of impulses making up a single group is released in response to one-half of the cycle of the sound-wave and no impulses in response to the second half of the cycle. This correspondence is illustrated schematically in Fig. 1.

The question may be raised, however, whether the acoustic nerve is capable of conducting impulses at a high enough frequency to provide, in terms of the schema suggested, all the frequency and intensity-steps that actually occur among our auditory experiences. Let us consider the conducting capacity of the acoustic nerve as a whole, taking into account the operation of the all-or-none law in each of its fibers. We may accept 0.02 sec. as a conservative estimate of the absolute refractory phase in any given nerve-fiber[4] and 13,000 as the number of fibers in

[3] E. D. Adrian, *The Basis of Sensation*, 1928.

[4] P. Broemser, Nervenleitungsgeschwindigkeit, Ermüdbarkeit und elektrotonische Erregbarkeitsänderungen des Nerven, Bethe's *Handbuch der normalen und pathologischen Physiol.*, 1929, Vol. IX, 220; also H. Rosenberg and K. Kitayama, Untersuchungen über Nervenaktionsströme, IV, Aktionsströme bei natürlicher Erregung (Strychninkrampf), *Arch. f. d. ges. Physiol.*, 223, 1930, 723.

the nerve. The acoustic nerve should then be capable of conducting a maximum of 13,000 × 200 or 2,600,000 impulses per sec. At an auditory frequency of 20,000 cycles per sec. (a figure that may be accepted as approximating the maximum audible frequency), each group of nerve-impulses (corresponding to one-half a cycle of the sound-wave) might contain a maximum of 2,600,000/(20,000×2) or 65 impulses. A succession of such groups might serve as the correlate of the maximum intensity (at an auditory frequency of 20,000); a succession of groups containing fewer impulses, the correlate of lesser intensities. By variation of the number of impulses in the groups, the possibility is thus provided, theoretically, of 65 intensity-steps. We are not familiar with any investigation made to determine the number of intensity-steps at such high frequencies, but the fact that 20,000 cycles per sec. is given as the approximate upper limit of hearing indicates that there must be far less than 65 steps at this frequency, if, indeed, it can be heard. In the middle region of the frequency-range, there are the thoroughgoing experiments of Knudson.[5] At a frequency of 1000, which lies in the frequency region of greatest sensitivity, he estimates the existence of 400 intensity-steps under the most favorable conditions for hearing. A calculation of the number of impulse-units possible in groups occurring at a frequency of 1000 per sec. shows that each group might contain a maximum of 2,600,000/(1000 × 2) or 1300 impulse-units, thus providing the possibility of 1300 intensity-steps at this frequency, about three times as many as actually found. It is evident, then, that the acoustic nerve, regarded as a whole, is more capable in terms of the hypothesis suggested here of providing for the frequency and intensive steps found among our auditory experiences.

The characteristics of the acoustic nerve response pictured here implies, of course, a particular type of functioning on the part of the auditory receptor-mechanism producing the nerve-currents and of the cortical mechanism which responds to them.

The cortical mechanism must be able to respond summatively to the successive impulses within a group and with a specific kind of reaction to the frequency of the groups. That is to say, the specific kind of cortical response correlated with the frequency of the groups (and the corresponding sound-quality) must vary in its magnitude with the frequency of the impulses within the groups. A mechanical analogy for such a performance would occur in the response of a vibrating body in resonance with the frequency of successive groups of 'pushes,' but with a period longer than the time-intervals between the individual pushes within the groups. Such a body would respond with the specific frequency of the successive groups and summatively to the separate pushes making up the groups. That 'summative mechanisms' occur in the physiological organism is demonstrated by the phenomenon of summation of muscular responses, and that like devices exist cortically is indicated by the correlation between intensity of stimulus (and presumably of intensity of sensation) and frequency of nerve-impulse found by Adrian. This correlation could not exist in the absence of cortical mechanisms which respond summatively to the frequency of the impulses. In

[5] V. O. Knudson, The sensibility of the ear to small differences of intensity and frequency, *Phys. Rev.*, 21, 1923, 92; also M. Gildermeister, Hörschwellen und Hörgrenzen, Bethe's *Handbuch der normalen und pathologischen Physiol.*, 11, 1926, 535-546.

order that the magnitude of response of such a summating mechanism vary with frequency of impulse, it is, to be sure, not necessary for the neural intensity-units which are being summated to follow each other at regular intervals. They need only fall within the temporal period within which summation occurs.

The present hypothesis also implies a certain type of performance on the part of the receptor mechanism, namely, a performance that will produce groups of impulses at regular intervals corresponding to the frequency of the sound-waves, and variations in the frequency of the impulses, in the group, corresponding to the intensity of the stimulus. The basilar membrane, we may suppose, will take up a sequence of positions corresponding to the pressure changes in the impressed

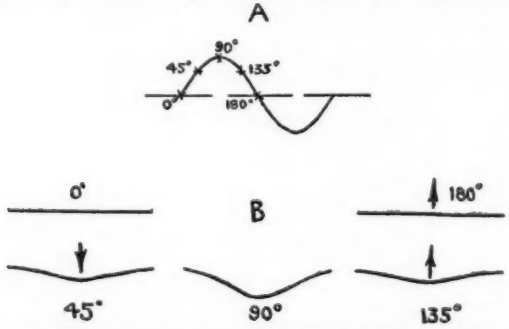


FIG. 2. SHOWING RELATION BETWEEN CYCLE OF THE STIMULATING SOUND-WAVE AND THE DISPLACEMENT OF THE BASILAR MEMBRANE

A represents the cycle of the stimulating sound-wave, and B the displacement of the basilar membrane. The arrows indicate the direction of the movement of the membrane.

sound-wave. As the sound-wave proceeds in its cycle from a phase of 0° to a phase of 180°, the basilar membrane, or some portion of it,[6] will move from its median position to its position of maximal displacement (at which point it corresponds to a phase of 90°) and then return again to its median position (see Fig. 2). Corresponding to the second half of the wave-cycle, the basilar membrane may go through a similar cycle of positions on the opposite side of its median position. As the membrane moves from its median position it will exert a tension on the connected hair-cells, the magnitude of this tension depending on the distance of displacement of the membrane. It does not seem unreasonable to suppose that the hair-cells can discharge and give raise to neural impulses only when the tension exerted upon them comes from a certain direction, when, *e.g.* the membrane is displaced downwards. This suffices for the formation of impulse-units (groups) whose frequency will correspond to the frequency of the sound-wave, for impulses will occur during one-half of the cycle of the wave and no impulses during the other half. But just when in the cycle of the sound-wave may these impulses be expected to occur? We may suppose that a certain minimal displacement of the

[6]For the purposes of our discussion, it makes no difference whether one supposes that the basilar membrane vibrates as a whole, for all frequencies, or that its place of vibration, or place of maximum amplitude, varies for different frequencies.

basilar membrane is necessary before the tension exerted on some hair-cell in the affected region is great enough to cause that cell to discharge. But as the displacement of the membrane proceeds further, the tension on the cells already under tension will increase, and cells not previously under tension will begin to be affected (due to the spread of the displacement further along the extent of the membrane). So we may expect further neural discharges to occur, both from those cells which were already under tension but which required a somewhat higher tension to elicit discharge, and from cells of maximal sensitivity whose thresholds have just been reached due to the lateral spread of the basilar displacement. Moreover, cells which have already discharged may again do so, within the limits imposed by their refractory period. So the running through of the cycle of the sound-wave will be "duplicated' by the magnitude of the average tension exerted on the hair cells, and this in turn, by the density or frequency of neural discharge. It is, of course, true that there may be some distortions or asymmetries produced in the distribution of nerve impulses along the half-cycle period, due to the refractory phase of fibers which have already discharged. For more discharges, even for like positions of the basilar membrane, are likely to occur at the earlier than at the later phase of the cycle, at least for the first few cycles. It is also true that the exact temporal relations of the successive impulses will depend on the distribution of threshold values among the hair-cells of any affected region. This may result in certain asymmetries in the 'copying' of the wave-form by the density or frequency of the impulses, but should not, in so far as the magnitude of tension on the hair cells is a determining factor, eliminate the correspondence existing between the two orders (that is, between the wave-form and the neural response).

Now, on the basis of such a 'discharge mechanism,' what differences may we expect in the neural responses corresponding to *different* stimulus-intensities? We may expect, first of all, that for stimuli *just* above the threshold, the times of neural discharge will correspond to the crests of the sound-wave; for the basilar membrane will exert its maximal tension upon the hair-cells at this instant. But for stimuli of greater amplitude, the basilar membrane will produce this degree of tension, the minimum necessary for the most sensitive cell to discharge, *earlier* in its cycle, and therefore the impulses making up the group will be spread out on both sides of the time-position corresponding to a phase of 90°. Our discussion shows then the possibility of a 'copying' of the relations in the sound-waves by the nerve-currents; further, that for stimuli of low intensities the 'copying' will occur for the crests of the waves, and that as the wave increases in amplitude the copying will extend out to neighboring phases of the waves.

The above discussion also suggests a serious flaw in a principle quite generally adopted by previous 'frequency theories' of audition, *i.e.* by those theories which suggest that the frequency of the stimulating wave is reproduced in the neural response. Many of these theories assume that as the basilar membrane, or parts of it, vibrates back and forth in time with the phases of the sound-wave, neural discharge will occur at a *particular* phase of the cycle,[7] *e.g.* corresponding to the

[7]See, for example, the hypotheses of Banister, Troland, Fletcher and those listed under the heading of "the remaining possibilities" by Wever and Bray. H. Banister, A suggestion towards a new hypothesis regarding the localization of sound, *Brit. J. Psychol.*, 17, 1927, 145; L. T. Troland, The psychophysiology of auditory qualities and attributes, *J. General Psychol.*, 2, 1929, 44; H. Fletcher, *Speech and Hearing*, 1929; E. G. Wever and C. W. Bray, Present possibilities for auditory theory, *Psychol. Rev.*, 37, 1930, 374-380.

crests (phase of 90°). But no mechanism is proposed by which this will come about, and it is difficult to imagine a plausible mechanism for bringing it about. If it is supposed that a given hair-cell (which is in a rested condition) will discharge as soon as it is under a certain necessary degree of tension, then it is obvious that this degree of tension *will not occur* at the *same* phase position for sound-waves of different amplitude. For a weak sound-stimulus, the necessary degree of tension may occur only for that position of the basilar membrane corresponding to a phase of 90°, but for stimuli of greater amplitude it would necessarily occur earlier in the cycle. This criticism seems to us to hold regardless of the particular phase angle that is suggested as the discharge position.

If it is further assumed that different nerve-fibers in a given region have different thresholds, then a wave whose amplitude is great enough to cause fibers of different thresholds to discharge cannot produce these discharges at the same instant. For, the displacement of the basilar membrane sufficient to cause the more sensitive fibers to discharge will be reached earlier than the displacement necessary to stimulate the less sensitive fibers. The discharges produced within a single cycle of the stimulating wave must consequently be spread out in time. These conclusions are incompatible with either of the following two doctrines commonly found in recent theories: (1) that the neural discharge always occurs at a particular phase position, (2) that the neural discharges (from coöperating fibers) produced during a single cycle occur at the same time. The acceptance of the premises underlying our conclusions means, then, the rejection of the two doctrines just mentioned and the adoption, instead, of a frequency hypothesis of the sort proposed in the present paper.

University of Berlin GEORGE KREEZER

CERTAIN ATTITUDES OF PRESENT-DAY PHYSICISTS AND PSYCHOLOGISTS

A well-known psychologist remarked recently that at the present time, among all the sciences, physics rests upon the most insecure foundations; that of all scientists, the physicist is least sure of the validity of his science. This speaker seemed to find consolation in his belief that, as compared with physics, psychology was firmly established and relatively secure.[1] Psychology is in reality a sort of superstructure that looks to other sciences, to physiology, neurology, zoölogy, endocrinology, etc., for many of its fundamental postulates. And these sciences turn to chemistry and to physics for their foundations; and physics in turn looks to mathematics for certain of its basic principles. In this hierarchy of the natural sciences, the superstructures are obviously no more valid than are the basic ones.

A conspicuous difference between the psychologist of 1931 and the physicist of 1931 is thus revealed. The physicist of 1931 occupies a substructure; the psychologist, a superstructure. The physicist admits that he is bewildered and mystified by the phenomena that he studies; the psychologist, however, often denies that the phenomena which he observes are due to unknown or unanalyzable agencies. Evidence in support of the two foregoing assertions follows.

[1] This remark is particularly interesting to those who witnessed the fervid attempts of certain psychologists only a few years ago to make psychology as objective as physics.

Attitudes of many present-day physicists. Not long ago people believed somewhat generally that the leaders in physical science had discovered unalterable laws, and that it was possible to explain almost all natural phenomena in terms of physical agents operating according to these laws. The physicists were among the first to introduce and to propagate a mechanistic concept of law and order in the world. Workers in other fields, including certain physiologists and most behaviorists in psychology, adopted without qualification a mechanistic point of view. Many so-called laws were formulated. The psychologist often rationalized his extremely behavioristic and mechanistic approach by presenting an analogy between his method and that of the physicist. The attitude of the physicist today, however, is decidedly changed. A physicist (Heisenberg) has introduced recently the "principle of uncertainty," a principle which, according to Millikan, "is causing . . . much excitement among physicists just now."[2]

This principle is alleged to rule the microscopic universe. Physicists formerly conceived matter to be an agglomeration of hard and solid particles, and they developed "laws" which made possible numerous predictions. For example, if one knew the exact location of a baseball and its rate of progress, one could predict accurately the continuous future position of the ball. For practical purposes, this and similar laws were found to be quite valid; predictions were made and verified. Many physicists, however, felt that laws had been developed by which prognoses could be made with a considerable degree of accuracy for *all* matter. By some individuals, mechanistic control and prediction were assumed to hold for the animate and the inanimate, for the visible and the microscopic. One difficulty with the acceptance of this credo is the recently ascertained fact that in microscopic processes the mechanistic laws are not applicable. Certain laws which operate in the activity of finite bodies cannot be applied to the infinitesimal. The physicist today frankly admits his dilemma, and acknowledges readily the crudity of his predictions. What is more important, he admits the lack of the *general applicability* of his principles.

The physicist of 1931 discards many laws. Of the six basic principles which at the end of the nineteenth century were assumed to have been established for all time,

(1) The principle of the conservation of the chemical elements,
(2) The principle of the conservation of mass,
(3) The principle of the conservation of energy,
(4) The principle of the conservation of momentum,
(5) The principle underlying Maxwell's electrodynamics,
(6) The principle of entropy or the second law of thermodynamics,

Millikan writes "there is not one the universal validity of which has not been questioned recently by competent physicists, while most of them have been definitely proved to be subject to exceptions."[3]

In view of the foregoing declarations it is not surprising that the present-day physicist hastens to admit his insecurity.

"Nowadays whenever enthusiasts meet together to discuss theoretical physics the talk sooner or later turns in a certain direction. You leave them conversing on their special problems or the latest discoveries; but return after an hour and it is any odds that they will have reached an all-engrossing topic—the desperate

[2]R. A. Millikan, *Science and the New Civilization*, 1930, 180.
[3]*Op. cit.*, 155.

state of their ignorance. This is not a pose. It is not even scientific modesty, because the attitude is often one of naïve surprise that Nature should have hidden her fundamental secret successfully from such powerful intellects as ours. It is simply that we have turned a corner in the path of progress and our ignorance stands revealed before us, appalling and insistent. There is something radically wrong with the present fundamental conceptions of physics and we do not see how to set it right."[4]

The following quotations present additional evidence of the insecurity of the physicist.

"Strict causality is abandoned in the material world. . . . All the indications are that strict causality has dropped out permanently."[5]

"The law of gravitation is a put-up job."[6]

"The view, which some of us were taught at school, that the truth of Euclid's axioms can be seen intuitively, is universally rejected nowadays. We can no more settle the laws of space by intuition than we can settle the laws of heredity."

"At the present time there is no sense in which the quantity of matter in a pound of lead can be said to be equal to the quantity of a pound of sugar. Einstein's theory makes a clean sweep of these pious opinions."[7]

"On the one side there is consciousness stirring with activity of thought and sensation; on the other side there is a material brain.

"Let us for a moment consider the most crudely materialistic view of this connection. It would be that the dance of atoms in the brain really constitutes the thought, that in our search for reality we should replace the thinking mind by a system of physical objects and forces, and that by so doing we strip away an illusory part of our experience and reveal the essential truth which it so strangely disguises. I do not know whether this view is still held to any extent in scientific circles, but I think it may be said that it is entirely out of keeping with recent changes of thought as to the fundamental principles of physics."

"Physical science has turned its back on all such mechanical models, regarding them now rather as a hindrance to the apprehension of the truth behind the phenomena. We have the same desire as of old to get to the bottom of things, but the ideal of what constitutes a scientific explanation has changed almost beyond recognition. And if today you ask a physicist what he has finally made out of the aether or the electron, the answer will not be a description in terms of billiard balls or fly-wheels or anything concrete; he will point instead to a number of symbols and a set of mathematical equations which they satisfy. What do the symbols stand for? The mysterious reply is given that physics is indifferent to that; it has no means of probing beneath the symbolism. To understand the phenomena of the physical world it is necessary to know the equations which the symbols obey but not the nature of that which is being symbolized."[8]

Another physicist, speaking of the trend of modern physical science, asserts that "every conclusion . . . is quite frankly speculative and uncertain."[9]

Attitudes of many contemporary psychologists. In contrast with the physicist of 1931, who, according to Millikan,[10] has had the limitation of his knowledge jolted into him enough times to believe it, the psychologist often accepts unverified hypotheses and even refers to them as laws. Many current texts in psychology present new and unique laws. The organismic laws recently heralded by Gestaltists are interesting examples. In numerous instances, the psychologist

[4]A. S. Eddington, *The Nature of the Physical World*, 1929, 179.
[5]Eddington, *op. cit.*, 332. [6]*Ibid.*, 143. [7]*Ibid.*, 159, 255.
[8]Eddington, *Science and the Unseen World*, 1929, 27 f., 29 f.
[9]J. H. Jeans, *The Mysterious Universe*, 1930. [10]*Op. cit.*, 180.

does not verify by repeated experimentation the laws which he assumes to have general applicability. Not only are laws propounded for general psychology, but principles and predictive devices abound in applied psychology.

In the *Science News Letter* of November 29, 1930, there is an article entitled, "Uncle Sam Becomes Vocational Counselor," which states that the government is now providing a new approach to the unemployment problem. With specially devised tests, the U. S. Civil Service Commission will guide high school students into the work for which they have the most talent. The Commission will also direct pupils to work in which they will have an excellent chance to be happy and successful. The remarkable and scientific innovation, made possible by modern psychology, follows.

A list of some fifty occupations is presented with the average test scores earned by self-styled experienced members of each occupational group upon certain tests. The following remarks explain the value of this list.

"The figures show the *average* ability of people already engaged in the occupations listed. *Your own score would indicate the type of work you are best fitted for.* This table was compiled from results of the U. S. Army psychological tests made during the World War and is similar to the guidance card the government is now planning to furnish high school pupils."[11]

The complacency of the originator of the practice recommended above should be noted. It is of course ridiculous to assume that, within occupational groups, individual differences of any kind can be dismissed in such a brusque manner. Some workers will always be above the average; others will be below the average, regardless of the trait considered or of the validity of the test. Certainly, few careful students who are familiar with the Army tests would place much credence in them as occupational guides.

As vocational counselor, the U. S. Civil Service Commission seems to take no account of the fact that the *range* of test scores within any occupational group is large, so large that the average hides about as much as it reveals. Moreover, no data are available which demonstrate that those individuals who earn *average* scores really have the best chance of finding happiness and success in a given occupational endeavor.

The article in the *Science News Letter* is a very fair sample of the questionable postulates and suggestions that one encounters when one examines the literature that has to do with vocational counseling. This so-called science is overburdened with hypotheses which the workers make little or no effort to validate.

Another example of the unwarranted procedure of the applied psychologist is encountered in many of the reports in the *27th Yearbook of the National Society for the Study of Education* (*Part I*). Intricate and certainly inappropriate mathematical formulae were used to assign values to the relative effects of inheritance and environment upon intelligence. The workers arrived at different conclusions; some insist that the statistical data prove the superior potency of environment in affecting ability; others that heredity is the important factor. The difficulty in all these investigations is that it is impossible to separate precisely environmental and hereditary factors. Nevertheless, several psychologists appear quite willing to assign exact values to the relative influence of the two variables.

[11]M. Vandewater, Uncle Sam becomes vocational counselor, *Science News Letter*, 18, 1930, 342-343 (italics ours).

Numerous other examples of what is known by certain modern psychologists might be cited, but the foregoing are perhaps sufficient to illustrate a crucial difference between the psychologist of 1931 and the physicist of 1931. The psychologist of 1931 certainly is on ground that is no better known than is that of the physicist, but he often exhibits extraordinary self-confidence. Is it possible that the psychologist's attitude is due in part to the effort to compensate for the realization of the fact that his methods are inferior to the methods of certain other scientists? The physicist knows that his methods are the best that the mind of man has conceived, and the psychologist surely is employing inferior and often unreliable technic.

The physicist's attitude toward behavior problems. It is a most interesting fact that although the physicist readily admits that he does not understand "the nature of that which is being symbolized," he nevertheless insists upon describing "that which is being symbolized" when he discusses mental activity.

"To put the conclusion crudely—the stuff of the world is mind-stuff. . . . The mind-stuff of the world is, of course, something more general than our individual conscious minds; but we may think of its nature as not altogether foreign to the feelings in our consciousness.[12]

"I assume that we have left the illusion of substance so far behind that the word 'stuff' will not cause any misapprehension. I certainly do not intend to materialize or substantialize mind."[13]

It is indeed a curious fact that the physicist is exceedingly modest when he speaks as an expert, for in the realm of physics he admits that his knowledge is restricted. Nevertheless when he steps outside his own field he becomes bold. He assumes the rôle of expert psychologist, a rôle in which he can be only a rank amateur. He "explains" the "stuff of the world"; it is "mind-stuff." The foregoing situation seems to be due in part to the following fact. The physicist is incapable of explaining the intrinsic nature of things in terms of physical laws and agencies. He therefore "explains" it in terms of "mind," a datum which lies outside the physicist's experimental experience.[14]

The psychologist's attitude toward behavior problems. It is of interest that whereas certain physicists strive to explain matter in terms of "mind-stuff," psychologists often attempt to explain drives, motivation, thinking, etc., by objective physical elements or processes. It is common today for certain psychologists to explain much of human activity in terms of tissue needs, endocrine activity, and physiological stresses. The psychologist obviously can know relatively little about these matters as a direct result of his experimentation. Nevertheless the following statements probably would be agreed upon by many psychologists.

"We have traced the original sources of the motivation and energizing of human behavior back to tissue conditions of the organism."

"A good deal of mystery has been needlessly attached to the speaking that may be going on in a person silently. Just because it is inaudible and invisible to an attentive neighbor we need not jump to the conclusion that some new non-physical process or some new non-material entity is at work. Calling it a

[12]Eddington, *The Nature of the Physical World*, 1929, 267. [13]*Ibid.*, 180.

[14]Here again Jeans takes practically the same position as Eddington, asserting that to him "the universe begins to look more like a great thought than a great machine" (*op. cit.*).

'psychic' process, or the working of 'the mind,' only adds to our problems. It explains nothing. As natural scientists our quest is a search for mechanisms and events that can be described in the terms of natural science—physical and objective things and processes."

"These analyses of the thinking process are decidedly hypothetical, but they are offered the reader to remind him of the possibility of adequate mechanistic and natural science explanations in terms of physical relations and physical behavior, with no necessity of calling in a *deus ex machina* from any non-material realm. Thinking is a physical process going on in a physical world. Here, as at certain other points in our survey of man's behavior, the need now is for scientific experimentation to determine which of the many possible physical explanations adequately describe what does go on."[15]

Here, surely, is an interesting difference between the psychologist and the physicist. Each seeks his ultimates in the other's field. The physicist strives to explain the intrinsic nature of things in terms of "mind-stuff"; the psychologist attempts to explain the intrinsic nature of human behavior in terms of matter, or by reference to physical and objective things and processes.

And this difference—is it a mere difference in terminology? Do the physicist and the psychologist employ different words which mean the same thing? It will be granted readily that the difference may be in part one of mutual understanding of terminology. Surely it would be absurd to insist that when the physicist uses the term "mind-stuff," he means what the psychologist does when he speaks of "matter." Their divergence is partly caused by the fact that they have very different experimental experiences and consequent attitudes.

Efforts to maintain consistent sciences of physics and of psychology. In his presidential address before the Glasgow meeting of the British Association for the Advancement of Science, William Bragg spoke recently as follows:—

"There is a problem of which the minds of physicists have been full in recent years. The nineteenth century theory of radiation asks us to look on light as a series of waves in an all-pervading ether. The theory has been marvelously successful. . . . . There is no question of its truth in the ordinary sense.

"In the last twenty or thirty years a vast new field of optical research has been opened up, and among the curious things we have found is the fact that light has the properties of a stream of very minute particles. A wave theory is of no use in the newer field. How are the two views to be reconciled? How can anything be at once a wave and a particle?"[16]

In Einstein's equation, matter and ether waves are fused so that ether and matter become indistinguishable terms. Electrons are both particles and waves, and light waves are also particles. Because the wave theory and the particle theory appear irreconcilable, the physicist finds himself in distress. It surely is disconcerting for a scientist who has felt that he knew the real nature of the physical world to be faced suddenly with the fact that his basic laws are not of general applicability or of fundamental validity. How is the difficulty to be bridged? Bragg resorts to a very common means of escape.

"To repeat a phrase which I employed a few years ago in addressing a university audience familiar with lecture time-tables, on Mondays, Wednesdays,

[15] J. F. Dashiell, *Fundamentals of Objective Psychology*, 1928, 248, 483, 518.

[16] W. Bragg, quoted in Science and Craftsmanship, an unsigned article in *Science News Letter*, 14, 1928, 276.

and Fridays we adopt the one hypothesis, on Tuesdays, Thursdays, and Saturdays the other."[17]

Millikan points to the fact that, although as yet the two contradictory theories (the particle and the wave theories) have not been reconciled, it is necessary that a reconciliation be effected. But how? Millikan confesses frankly that he does not yet see just how this is to be accomplished.

"I am not worrying here over the recent introduction of the so-called 'principle of uncertainty' in microscopic processes, an event that is causing so much excitement among physicists just now. This may indeed be consoling or, at least, illuminating to those non-physicists who have been worrying their heads over their inability to reconcile the principle of law with the facts of free-will and of responsibility. We physicists have had much worse contradictions than these to put up with in the subject of physics alone, as for example, the reconciliation of the wave theory of light with the essentially corpuscular light-quant theory. Experiment has told us that both theories are right, and we have had the limitations of our knowledge jolted into us enough times lately in physics to believe it, in spite of our inability to see as yet just how the reconciliation is to be made."[18]

Because the reconciliation has not been brought about, physicists have for the past fifteen years been employing both classical laws and quantum laws, in spite of their irreconcilability.[19] The physicist, however, is willing to admit this paradox.

Physicists are unable to maintain consistency and they recognize the fact frankly. In contrast to this attitude, psychologists insist above all else upon theoretical implications which are consistent. Many psychologists regard lack of consistency as a cardinal sin.

Thus, the psychologist of 1931 strives tenaciously to maintain a consistent point of view. He does this by allying himself with a so-called 'school' of psychology. Each school attempts to be consistent. Each maintains internal consistency by minimizing the importance of or by ignoring utterly the existence of numerous important experimental facts. Physicists are less able to do this than are psychologists; they appear unable to establish 'schools' of physics. Unlike the physicist, the psychologist is able, through the establishment of 'schools,' to maintain consistency (of a kind).

The psychologist of 1931 thus finds himself in a situation almost opposite to that of the physicist. In the first place, even within his own field of investigation, the psychologist has accumulated only fragmentary data. This statement is not a reflection upon psychologists either as students or as research workers. The existing situation is due simply to facts such as the following. Psychology is a relatively immature science. The subject matter is exceedingly complicated, and the cause-and-effect relationships are therefore intricate and subtle. Psychology depends upon several supporting sciences. Thus, when one type of psychologist cannot 'analyze further' he is able casually to shift his responsibility. An example of how this may be done follows.

"The sources of human motivation given in the foregoing account have been referred to as 'primary' sources; but they are primary only for the psychologist, and they remain legitimate objects of further analysis by any physiologist or physiological chemist. *No mystery enshrouds them.*"[20]

[17]Bragg, *loc. cit.*, 276.
[18]Millikan, *Science and the New Civilization*, 1930, 180.
[19]Eddington, *The Nature of the Physical World*, 1929, 194.
[20]Dashiell, *Fundamentals of Objective Psychology*, 1928, 246. (Italics ours.)

Because the psychologist is able to end his analysis at a given point,[21] and because he feels no responsibility for analyzing his data completely, he sees no mystery in basic elements. Because the physicist is unable to shift the responsibility for complete analysis, and because he is incapable of analyzing his data reliably, he sees mystery everywhere.

The psychologist's attitude toward the mysterious. The complacency of the psychologist is well illustrated in the following quotations. A presidential address which was delivered before the Ninth International Congress of Psychology at New Haven, Conn., Sept. 4, 1929, included the following remarks.

"This brings us to the question of the *mechanism* by which organization of behavior is brought about. There is, I think, *nothing mysterious* about the problem. . . . There is certainly no need to look for non-physical agencies."[22]

Note that in the foregoing quotation the psychologist denies that he is confronted with a mystery. He also discards the concept of non-physical causal factors, implying that the phenomenon in question is due to physical agencies. This tendency on the part of the psychologist to demand objectivity is very widespread.

The following excerpt is taken from a letter that was received by one of the writers from a book company. The book company was anxious to make a prospective purchaser realize how simple psychology can be made. The company therefore presented by way of illustration a few sentences from the book.

"There is *nothing mysterious* about the process by which we become assured of the existence of other minds. It is simply [sic!] the *normal process* of adjustment to social situations, of coming into psychological adjustment with one's fellows. It is nothing more than *common sense*, which we have only tried to *explain and justify.*"[23]

The terms 'mystery,' 'mysterious,' etc., are much used by modern psychologists, chiefly in connection with refutation.

"If we add another experimenter who is to observe both the subject's behavior and the behavior of the first experimenter, *no mysterious* element enters."[24] (Italics ours.)

A highly approved and widely used textbook in general psychology was scanned for the purpose of discovering the frequency with which 'mystery' is denied.[25] The terms 'mystery,' 'mysterious,' and 'mystical,' were found to occur very often. These words were employed usually in denying that there was any mystery about the phenomena which were being discussed. The attention of the reader will doubtless be attracted to the oft-repeated tendency to 'explain' the psychological phenomena by 'physical agencies.'

"Let it be noted that there is *nothing mysterious* in this questioning method," *i.e.* self-observation and verbal report (11). "Nor should the word 'internal' here be given any *mystical* flavor. There is nothing sacrosanct or inscrutable about

[21]One group of present-day psychologists feels little or no need for analysis. The organismic group asserts that its general principles of whole-activity account satisfactorily for all behavior manifestations.

[22]K. S. Lashley, Basal neural mechanisms in behavior, *Psychol. Rev.*, 37, 1930, 1-24. (Italics ours.)

[23]W. W. Spencer, *Our Knowledge of Other Minds*, 1930. (Italics ours.)

[24]W. S. Hunter, *Human Behavior*, 1928, 14.

[25]Dashiell, *Fundamentals of Objective Psychology*, 1928.

the energies generated by processes occurring in heart, lungs, blood vessels, intestines, endocrine glands, striped muscles, or nerve tissues" (32). "So man to-day can attain control of the human equation by seeking explanation in terms *not of mystical* powers hidden away in the brain, but of *physical processes* operating there and elsewhere" (138). "There is *nothing mysterious* or inevitable about a plateau [in the learning curve]; it is simply . . . " (353). "Transfer, then, was shown *not* to be any *mysterious* principle of learning, but to be . . ." (354). "there is *no mystery* about the fact that the manner in which a person sizes up a thing or situation confronting him may lead him astray, because of an illusion" (399). "There is *no* essential *mystery* about the effects of other people upon a person's activity" (437). "By this time the reader should realize that illusions are *not* to be looked upon as essentially *mysterious*" (401). (Italics ours.)

In the foregoing quotation Dashiell insists that there is *no mystery* about the fact that we experience illusions. In the following paragraph Eddington asserts that it is a mystery that we are capable of perceiving visual images at all.

"We all share the strange delusion that a lump of matter is something whose general nature is easily comprehensible whereas the nature of the human spirit in unfathomable. But consider how our supposed acquaintance with the lump of matter is attained. Some influence emanating from it plays on the extremity of a nerve, starting a series of physical and chemical changes which are propagated along the nerve to a brain-cell; there *a mystery happens*, and an image or sensation arises in the mind which cannot purport to resemble the stimulus which excites it."[26]

In the following statement a key to what the psychologist means when he employs the term "no mystery" is found.

"As natural scientists our quest is a search for *mechanisms* and events that can be described in the terms of natural science—*physical* and *objective* things and processes."[27]

When he can "explain" behavior trends in terms of *physical* and *objective* things and processes, the phenomena cease to be mysteries to the psychologist. The physicist on the other hand states frankly that explanations of conscious reactions in terms of *physical* things and processes are not explanations at all. To the physicist such integers as these are about the last ones in the world that may be employed for "explaining" the causes of human activity.

The physicist's attitude toward the mysterious. Because physics is a relatively mature science, the physicists have accumulated a relatively large amount of data. The physicist knows much within his own field of investigation. The physicist knows moreover that his methods of investigation are perhaps the best that the mind of man has conceived. Because of these facts, and because he is unable to turn to any supporting science (other than mathematics), the modern physicist finds himself both mystified and bewildered by the world of natural phenomena.

It is true that the physicist, as a scientist, sometimes explains phenomena in terms of mathematical symbols, but this type of explanation apparently does not fully satisfy him. One physicist admits his predicament in the following.

"From what has been said it will be clear, I think, that the modern atomic physicist is becoming more and more of a philosopher. This does not mean that he is losing touch with facts, but rather that he is coming more and more to

[26]Eddington, *Science and the Unseen World*, 1929, 33 f. (Italics ours.)
[27]Dashiell, *op. cit.*, 483.

realize that the data of atomic physics are so extremely complicated that without philosophical interpretation, *i.e.* the introduction of new and satisfactory hypotheses, they are meaningless."[28]

The dissatisfaction with the present-day possibilities of explaining the nature of man and the world in terms of atoms and electrons has led many physicists to become mystics. Sir James H. Jeans has written recently a book entitled *The Mysterious Universe*,[29] which presents the limitations of physics in interpreting the nature of the world. The writer admits frankly his inability to understand objectively the nature of the world and of man. The following quotations from Eddington and Millikan are also illustrative of the physicist's bewilderment.

"*Our bodies are more mysterious than our minds*—at least they would be, only that we can set the *mystery* on one side by the device of the cyclic scheme of physics, which enables us to study their phenomenal behavior without ever coming to grips with the underlying *mystery*."[30]

"In the passage I have quoted there is no direct reference to *religious mysticism*. . . . We cannot argue that because *natural mysticism* is universally admitted to some degree therefore *religious mysticism* must necessarily be admitted; but objections to *religious mysticism* lose their force if they can equally be turned against *natural mysticism*."[31]

"The physicist now regards his own external world in a way which I can only describe as *more mystical*, though not less exact and practical, than that which prevailed some years ago."[32] (Italics ours.)

"An atom is now an amazingly complicated *organism*, possessing many interrelated parts and exhibiting many functions and properties—energy properties, radiating properties, wave properties, and other properties quite as *mysterious* as any that used to masquerade under the name of 'mind,' so that the phrases, 'all is matter' and 'all is mind' have now become merely shibboleths devoid of meaning."[33]

"It is true enough that whether we are dealing with the experience of 'becoming' or with the more typical sense-experiences of light, sound, smell, etc., there must always be some point at which we lose sight of the physical entities ere they arise in new dress above our mental horizon. But if there is any experience in which *this mystery* of mental recognition can be interpreted as insight rather than image-building it should be the experience of 'becoming'; because in this case the elaborate nerve mechanism does not intervene. That which consciousness is reading off when it feels the passing moments lies just outside the door. Whereas, even if we had reason to regard our vivid impression of colour as insight, it could not be insight into the electrical waves, for these terminate at the retina far from the seat of consciousness."[34]

The quotations cited above are ample evidence that some physicists reject mechanistic doctrines.

Scientists and church membership. When one is confronted with mystery everywhere, one must find some means of escape through which complacency may be achieved. The psychologist, since he sees no mystery or transcendental force at work, feels little need for such escape. But the desire for a satisfactory

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[28]R. B. Lindsay, Some philosophical aspects of recent atomic theory, *Sci. Mo.*, 26, 1928, 299-305.
[29]Jeans, *The Mysterious Universe*, 1930.
[30]Eddington, *The Nature of the Physical World*, 1929, 277. (Italics ours.)
[31]Eddington, *Science and the Unseen World*, 1929, 46 f. (Italics ours).
[32]*The Nature of the Physical World*, 344. (Italics ours.)
[33]Millikan, *Science and the New Civilization*, 1930, 182. (Italics ours.)
[34]Eddington, *The Nature of the Physical World*, 1929, 89. (Italics ours.)

explanation of life phenomena must indeed be great for the physicist. A convenient means of achieving complacency is offered the physicist by numerous religious credos. Following the World War, Sir Oliver Lodge became an ardent protagonist for spiritualistic teaching. A British Nobel Prize winner in physics, Eddington, lectured in America on religion. Those who are familiar with the writings and public pronouncements of Millikan, Pupin, Compton, Jeans, and others, must have noted that, within recent years, the physicists have certainly supplied a considerable number of religious enthusiasts. Many of the most illustrious names in physics today may be cited by those of the clergy who wish to prove that religion and science go hand in hand.

It is difficult to measure religious faith. Any such attempt is crude at best. Nevertheless, the writers of this paper find that within the hierarchy of the sciences, one group appears to exhibit an unusual amount of religious faith. The evidence is the frequency with which scientists mention their religious affiliations when writing their biographical sketches for *Who's Who in America*. In providing biographical data for the 1926-1927 edition of *Who's Who in America* the candidates were asked to report "Religious denomination (if any)." The names of the most outstanding scientists were obtained from the 1927 edition of *American Men of Science*, the biographical directory edited by Professor James McKeen Cattell. This latter volume contains biographical sketches of approximately 13,500 individuals in the United States who have carried on research in the natural and exact sciences. Of these 13,500 research workers, those who have achieved notable success in their particular fields are identified by means of asterisks.

Cattell's explanation of how these eminent research men were recognized makes it perfectly clear that the scientists having asterisks or stars attached to the subjects of their research in the 1927 edition of *American Men of Science* were not selected arbitrarily. It is evident also that this list includes the most distinguished scientists in the United States.

There are 1423 'starred' names, 14% of whom were eliminated because they were not included in the 1926-1927 edition of *Who's Who*. Three percent also were discarded because of incomplete sketches in *Who's Who*, the names of the scientists in question being accompanied by little more than cross-references to an earlier edition of *Who's Who*. In all, 1189 reasonably complete biographical sketches were found and examined.

Of these 1189 sketches, 25% included information regarding church membership; 75% did not. It may be inferred either that those who neglected to state their church membership belong to no church, or that they did not consider the information of sufficient importance to include when they were preparing their biographical sketches. Absent-mindedness and forgetfulness can not be assigned as reasons for this neglect since, as previously mentioned, the questionary that is filled out by all those whose biographical sketches appear in *Who's Who* suggests specifically that "Religious denomination (if any)" be given.

Table I presents the percentages of individuals from several branches of science who stated their church affiliation for *Who's Who*. Ament reports that approximately 50% of 2,000 individuals selected at random (from this same edition

of *Who's Who*) reported denominational affiliation.[35] It is evident at once that the scientists reported church membership less often than did the unselected group. Only about 25% of the scientists give information regarding church affiliation. The fact that about 75% of the outstanding scientists do not mention affiliation with any church may be due in part to the fact that science is a very jealous mistress. Possibly the outstanding scientist is an individual who so busies himself with his studies that he neglects not only church activities but also fraternal, philanthropic, political, and social enterprises. The foregoing hypothesis may account in part for the fact that relatively few eminent scientists report church membership, but obviously this hypothesis can not account for the group differences that are here set forth.[36]

TABLE I

PERCENTAGES OF EMINENT SCIENTISTS WHO STATE THEIR CHURCH MEMBERSHIP WHEN PREPARING AUTOBIOGRAPHICAL SKETCHES FOR "WHO'S WHO IN AMERICA"

*No.**	*Branch of Science*	%
136.33	Physics	37.31
196.83	Chemistry	32.76
93.	Mathematics	29.57
73.5	Geology	25.52
53.	Astronomy	25.47
27.5	Engineering	25.45
123.67	Botany†	23.45
95.67	Zoology	21.95
34.5	Pathology	18.84
35.5	Medicine and surgery	18.31
55.17	Psychology	16.61
30.33	Biology	14.83
22.83	Anatomy	10.95
38.83	Physiology	9.27
1016.66	Average	25.43

*Fractions are here employed because certain scientists are specialists in two or more fields.

†Included under Botany are 21 plant physiologists and plant pathologists.

Of the various groups of scientists, the physicists and the chemists head the list in the frequency with which they designated church membership, and those scientists who deal more constantly and intimately with the human organism (physiologists, anatomists, and biologists) made note of their church affiliation

[35] W. S. Ament, Religion, education, and distinction, *School and Soc.*, 26, 1927, 399-406.

[36] The fact should perhaps be mentioned that the group differences between the scientists listed near the top and those listed near the bottom of Table I are not chance differences. Similar differences were found when the data were partitioned into halves and also when they were partitioned into fourths. Obviously the foregoing assertion does not apply to groups adjacent to each other in Table I.

much less frequently. The writers do not assert that conclusive data regarding church affiliation of scientists have been secured. There is nevertheless reason to assume that the data in Table I are indicative of religious faith or belief.

Table II sets forth some interesting age differences regarding the frequency with which scientists mention church membership. Table II shows the percentage of the oldest and of the youngest 25% of each of 14 groups of scientists who stated church affiliation. In 13 of the 14 groups *the oldest scientists mentioned religious denomination much less often than did the youngest ones.*

It should perhaps be stated at this point that age differences do not account for the differences between the entire group of scientists and the 2,000 individuals selected at random by Ament. The average age of the 1016 scientists was 57, the

TABLE II

PERCENTAGES OF OLDER AND OF YOUNGER EMINENT SCIENTISTS WHO INDICATE THEIR CHURCH MEMBERSHIP WHEN PREPARING AUTOBIOGRAPHICAL SKETCHES FOR "WHO'S WHO IN AMERICA"

No.	*Branch of Science*	*Older*	*Younger*	*Dif.*
136.33	Physics	7.39	68.65	61.26
196.83	Chemistry	21.70	39.94	18.24
93.	Mathematics	15.22	47.07	31.85
73.5	Geology	10.81	53.85	43.04
53.	Astronomy	13.04	56.00	42.96
27.5	Engineering	12.77	69.23	57.54
123.67	Botany	7.06	37.50	30.44
95.67	Zoology	8.27	52.57	44.30
34.5	Pathology	19.05	31.82	12.77
35.5	Medicine and surgery	.00	63.16	63.16
55.17	Psychology	7.32	29.17	21.85
30.33	Biology	12.77	.00	12.77
22.83	Anatomy	8.10	15.38	7.28
38.83	Physiology	.00	38.88	38.88
1016.66	Average	11.74	44.32	32.58

figure closest to the average of all notables included in the 1926-1927 edition of *Who's Who.* Nor do age differences account for the differences between the various kinds of scientists which are presented in Table I. The group differences in age did not appear to be either large or significant.

Several hypotheses might be adduced to account for the age differences presented in Table II. In the judgment of the writers these age differences may be attributed in part to the fact that prolonged human experience makes the worker in every field somewhat less credulous and certainly more critical of metaphysical beliefs and practices. Some evidence in support of the foregoing assertion was obtained by inspecting the biographies of 200 men selected from the 1926-1927 edition of *Who's Who.* The biographies were selected at random except that sketches of ministers were omitted. Of the youngest 25% of individuals thus selected, 52% indicated denominational affiliation; of the oldest 25%, only 24% stated their denominational affiliation.

That the young men, in almost all of the groups studied, consider it worth while to indicate their church affiliations is possibly indicative of their greater religious faith.

Regardless of the interpretation that may be placed upon age differences, it is the judgment of the writers that the data of Table I represent not merely the percentages from the various groups of scientists who thought it worth while to mention their religious denomination, but also the relative frequency with which the various groups take their church membership seriously. If the latter conclusion be valid, it is clear at once that the physicists consider church membership most seriously; the physiologists, the anatomists, and the biologists, less seriously. It is probable too that the older scientists look upon religious denominations less seriously than do younger ones. The reasons for the first of these two situations are doubtless very complicated. The following hypotheses or some combination thereof may explain in part the physicist's rather extreme interest in religion.

(1) In dealing with religious matters the physicist is far from his field of experimental study. Unable to explain the intrinsic nature of things in terms of matter and force, the young physicist accepts an explanation in terms of mysterious agencies, *e.g.* 'mind-stuff.' He appears willing in religion to accept hypotheses which do not appear valid to those more familiar with the human organism and human behavior.

(2) To the physiologists, the anatomists, and the biologists, the claims of certain religious denominations are palpably absurd.[37] They are not in accord with the law of parsimony which is applied by many scientists to behavior problems. Therefore, these men may not consider it worth while to mention religious affiliation (or to effect such affiliation).

(3) Psychologists are able to shift responsibility for complete analysis to neurology, endocrinology, physiological chemistry, etc. The psychologist asserts in effect that, although he himself is incapable of complete analysis of human behavior, he is able to name other scientists who hold the key to complete analysis. Having turned the problem over to others for solution the psychologist achieves relative complacency, a fact evidenced both by his infrequent church membership and also by his frequent assertion that no mystery enshrouds the problems of human behavior. Many psychologists are content with a mechanistic explanation of man and his universe. They see no mystery or transcendental force at work.

The psychologist's assertion that there is no mystery in life does not appear to satisfy the physicist. The physicist remains baffled, bewildered, and mystified by the complexity of all phases of life. Religion is for him therefore an easy escape. Nevertheless, the reader should bear in mind the fact that the older physicists do not mention church affiliation frequently, and that an amazingly larger number of *younger* ones do. The limitations of his science in explaining existence may cause the young physicist to react forcibly and to turn to compensatory explanations. Acquaintance with the world and with the limitations

[37] A. H. Compton, Immortality: four faculty men view the future. The physicist, *Univ. of Chicago Mag.*, 23, 1930, 5-10. See A. J. Carlson's rebuttal in the same issue; also his Science and the supernatural, *Science*, 73, 1931, 217-224.

of compensatory mechanisms may cause the older physicist to relinquish or modify his early concepts. Regardless of what the true explanation may be for these data, the reader must surely be impressed by the unusual differences in the reports of the several groups, and particularly by the attitude of the physicist, who, more often than any other scientist, considers religious affiliation an element essential in his biographical record.

Ohio University — Harvey C. Lehman
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Distance of Projection of the Visual Image in Relation to Its Apparent Intensity

In a recent paper by Dr. Ellis Freeman the following paragraph appears: "In their work on chromatic sensitivity, Ferree and Rand give values which are likely to be regarded as physical constants of the retina. All their observations were made at a stimulus-distance of 25 cm. The implication of their work, unconsciously based on the assumptions of geometrical optics, is that any other distance would give the same results provided the size of the stimulus were varied correspondingly. They have made determinations of the liminal intensities for spectral stimuli for the retina from fovea to extreme periphery. Since they have not dealt with the influence of stimulus-distance, it is to be taken for granted that they have assumed that this distance is indifferent provided the size of the stimulus is proportionately varied. They have, therefore, assumed that changes in external conditions will not influence the results so long as a constant image is delivered to the same retinal spot. That this classical assumption is untenable in relation to visual acuity, or the form sense, appears from work recently published."

We have the following comments to offer on this paragraph. (1) The fact that one selects a particular distance at which to work and specifies this distance by no means implies that distance is not regarded as a factor in the results obtained, particularly when the statement of the distance was not a part of the specification of the angle subtended by the stimulus at the eye. The implication in fact is quite the converse of this. If distance were considered as immaterial to the results, it would not be necessary to take the trouble to maintain it accurately constant at a given value and to specify that value as one of the conditions of work. (2) In the study referred to by Dr. Freeman our problem was to determine the effect of degree of eccentricity in the field on the value of the chromatic thresholds. This could be done only by holding constant distance and other variable factors. The fact that we did not in the same study determine also the effect of distance and all other possible factors, certainly can not be taken to mean that we did not recognize them as factors. Their recognition as factors was implied in the trouble and precautions that were taken to hold them constant. (3) Two years ago when Dr. Freeman visited our laboratory we advised him not only that we considered the distance of the object as a factor in the determination of retinal sensitivity, but more narrowly that we strongly suspected that the psychological factor, the distance to which the image is projected, affects the intensity of the sensation experienced. Further we discussed with him, in support of these views, experimental results which we had obtained nearly twenty years ago. (4) It seems strange to us that Dr. Freeman should fear that any one who has followed our work with a reasonable understanding of its content and purpose should re-

gard any results obtained by us as "physical constants of the retina," or should think that we regard them as such. If it were so, it would indeed be too bad, for the greater part of that work, so it seems to us, has been conscientiously devoted to showing or trying to show the relativity of results and the action and interaction of factors.[1] That we should thus be singled out as consciously or 'unconsciously' leading in dogmatism is indeed a tragic commentary on the success of our efforts.

However, the problem that is being studied by Dr. Freeman is not without merit. While not new, some aspects of the problem are worthy of more attention by psychologists than they have as yet perhaps received. That distance affects acuity has at least been fairly well known to ophthalmologists and workers in physiological optics for many years, but that it has an important or considerable influence on retinal sensitivity has received little attention other than what is implied in the control and specification of distance as one of the conditions of work.

It would seem that the possibilities of change of distance of the object or stimulus influencing the intensity of the retina's response may be summed up in the following effects: change in size of image, change in light density in image and change in distance of projection of image.

(*1*) *Change in size of image.* According to the laws of geometrical projection when the size of the object is kept constant and its distance varied, the size of the image formed on the retina should vary inversely as the square of the distance of the object; when, however, the size of the object is changed by the amount required to keep the visual angle constant, the size of the image should remain the same. But in both of these cases the change in the refraction situation is a modifying factor. That is, with change of accommodation the position of the second principal refracting plane or second nodal point, whichever one chooses to take as the point of reference, is changed, which should cause a change in the size of the image. With increase of accommodation the distance of this plane from the retina should become greater and the size of the image increase, and with decrease of accommodation the distance should decrease and the image become smaller. Size of image is supposed to have an effect on intensity of sensation within certain limitations as to range of change and position in the field, as well as on acuity. From this factor we might expect both acuity and sensory response to be affected by distance of the object.

(*2*) *Change in light density in image.* Change in distance of object causes a change in the size of the pupil and a proportional change in the amount of light condensed into the image. The flux of light through the pupil varies as the square of its diameter. From this factor alone a very considerable effect of distance on intensity of sensation should be expected if provision is not made to keep the flux of light constant through the use of an artificial pupil or in some other way. Apparently such provision was not made by Dr. Freeman in his experiments either on acuity or on the light sense. The effect on the light sense would come through a change in the intensity of the stimulation; and on acuity,

[1]This we had been doing long before we were made familiar with the term 'Gestalt.'

both through this and through clearness of imagery.[2] According to the laws of geometrical projection there should be no change in the light density in the image with a constant size of pupil because both the intensity of light incident on the pupil and the size of image vary inversely as the square of the distance of the object, *i.e.* these changes exert compensating effects on the density of light in the image. A compensating influence, within the limits of the cosine effect, is present also when the visual angle subtended by the object is held constant in making the change in distance. That is, in this case the intensity of light incident on the pupil varies inversely as the square of the distance but the size of the object (the source of light, primary or secondary) varies directly as the square of the distance. This situation, however, is modified by the change in size of image resulting from changes in accommodation as is noted in (*1*).

(*3*) *Change in the distance to which the image is projected.* This is a possible psychological factor and implies some relation between the intensity of sensation and the spatial magnitude of the projected image. As is well known, the apparent size of the image varies directly as the square of the distance to which it is projected. The question arises somewhat naturally, perhaps naïvely, does this spatial magnification, this spreading of the sensation over a larger surface, as it were, have an effect on its apparent intensity. If it does not, it so transcends all that is known of physical phenomena as to be another of the dualistic wonders of mind and matter which are so puzzling to workers in the borderline subjects. Our work on factors and their interaction would incline us to look for and expect more or less reciprocally an interplay or relativity between the spatial and sensory functions.

In 1914 two types of experiment were conducted by us on the effect of distance on the apparent brightness of objects.[3] Around this time somewhat exaggerated statements as to the independence of the apparent brightness of objects of any appreciable influence of distance were being made by certain photometrists engaged in the development of methods of measuring brightness. One of these writes: "One of the most striking characteristics of brightness is that a bright surface is equally bright whether one views it at 1, 10 or 50 m." In both types of experiment the determinations were made in a dark room with the eye dark-adapted for a period of 30 min. In the first type the threshold of achromatic sensitivity was determined for a surface of 45 sq. mm. at a distance of 1, 4, 7, and 10 m. In the second type the end point of the determination was a photometric balance, or a judgment of equality of brightness. In this experiment surfaces of the same size were used as test fields. One, the standard surface, was kept at a fixed distance and held constant in brightness. The distance of the other was varied and the density of light required to make it match the standard surface was determined for different distances. The match was made by successive judg-

[2]Objects are seen because of their size and their sensation difference from the background. With a constant difference in coefficient of reflection between object and background the sensation difference increases rapidly with increase of intensity of illumination. This is a very important, easily understood and easily demonstrated factor in the effect of intensity of illumination on the visibility of objects and on acuity. Whether there is in addition the direct effect on the space sense suggested by Professor S. Hecht (*J. Gen. Physiol.*, 11, 1928, 255-281), we are not at present prepared to judge.

[3]Cf. *Trans. Illum. Eng. Soc.*, 9, 1914, 192-195.

ments in order that changes in the size of pupil with distance and other factors pertaining to the adjustment for distance would have due play. In both sets of experiments a strong but irregular effect of distance as a factor was found.

In 1912 determinations were made under our direction of the effect of distance on the value of the chromatic threshold. Pigment stimuli under light adaptation and filter stimuli under dark adaptation were used. In these experiments the visual angle subtended by the stimulus was held constant as the distance was varied through a range from 1 to 14 m. In case of the pigment stimuli great care was taken to keep constant both the intensity of light incident on the stimulus and the state of adaptation of the eye of the observer. The effect of increase of distance here also was to decrease the apparent intensity of the response. In all these experiments the conditions were planned to give full effect to all of the following factors: change of accommodation as influencing both size of image and density of light in the image, change in size of pupil as influencing density of light in the image and sharpness of imagery, and distance to which the image was projected as a psychological factor. No attempt was made to isolate distance of projection of the image as a psychological factor.

In a study published by us in 1911 the existence of a narrow zone about the blind spot was reported, in which space values were greatly magnified.[4] We were concerned at this time to know whether this magnification of space values was a factor in the steepness of the sensitivity gradient immediately around the blind spot. As affording possible circumstantial evidence, other experiments were conducted in which subjective magnification of space values was produced in order to see whether in these cases, in which the amount of magnification could be subjected to variation and control, there was an effect of magnification on the intensity of the sensory response. One of the obvious ways of producing this magnification is the projection of the image to different distances. Either the positive image may be projected or the positive or negative after-image. The eye naturally projects the positive image to the distance of the object. Variation of distance of projection can be produced then by changing the distance of the object, as is the case if a threshold is obtained at different distances; or certain devices can be used to make the distance of projection greater than the distance of the object: (a) by looking beyond the object, as happens for example, when one looks between bars or through the meshes or a net or screen at some distant point; (b) by decreasing the angle of divergence of the rays of light entering the eye by means of a convex lens or a concave mirror; or (c) by decreasing the angle of convergence of the eyes by changing the placement of the images on the retina with prisms. As the most feasible of the above methods we decided to use the projection of the negative after-image of a colored stimulus. The effect of projection on the intensity of the sensory response was obtained by determining for the different distances of projection the intensity of stimulating light that was required to give a just noticeable after-image. The stimulus surface was kept at a constant distance from the eye and the after-images were projected successively in different sets of determinations to this distance and a series of greater distances. The time of stimulation was in all cases the same.

[4] C. E. Ferree and G. Rand, The spatial values of the visual field immediately surrounding the blind spot and the question of the associative filling-in of the blind spot, *Amer. J. Physiol.*, 29, 1912, 398-417.

The following conditions obtained in these experiments in subjective magnification. (*a*) During the stimulation the distance of the object and the accommodation were always the same; therefore the size of the image on the retina was always the same. (*b*) There was no effect of change of distance on the size of the pupil during the period of stimulation. Also there was no effect of change of brightness of the stimulus on the size of the pupil; that is, the saturation of the color was varied to give the different values needed for the threshold without change in brightness. During the period of stimulation, therefore, the pupil can be regarded as always having had the same or approximately the same size; and the image formed on the retina as having had the same or approximately the same intensity or density of light. (*c*) The projection fields were all carefully matched in brightness. In order that there should be no variable effect in the brightness of the images formed by these projection surfaces due to change in size of pupil with accommodation, which would affect the intensity of the projected after-image of the stimulus, the matching of brightness of these fields was made in successive judgments; that is, whatever variable effect was due to change of accommodation was taken account of in the visual act of matching. (*d*) The determinations were made in one of our specially lighted optics rooms. The intensity of illumination on the test object was 7 foot-candles, horizontal component. Supplementary illumination, variable as needed, was used for the illumination of the projection surfaces. The illumination was all of daylight composition and color. (*e*) The variable saturation of stimulus was obtained by combining on a color mixer a pigment sector with a sector of a gray of the brightness of the color. The color mixer could be swung out of the way in order to allow for the projection of the after-image without moving the eye. Several observers were used and a number of determinations were made for each distance of projection of the image. Also the series was repeated for each observer on different days. A well marked effect of distance of projection on the value of the after-image threshold was obtained. The amount of light required to arouse the after-image was found to increase as the distance of projection of the image was increased. This effect, we believe, was almost if not entirely free from any influence of changes in the size of image or density of light in the image; that is, it was almost, if not entirely an effect of distance of projection of the image on its apparent intensity.[5]

There seems then in general to be an effect of distance as a factor in determination of retinal responses even when narrowed down to the distance of the projection of the image. Whether or not there is a differential effect for the different colors remains yet to be determined. Just because we have recognized distance as a factor we have endeavored to standardize the diagnostic work in perimetry for a given distance. In case of our perimeter, for example, provision is made that the work on the arc and on the tangent screen shall all be done at the same distance. Diagnostic scales are compiled for this distance; and fields, blind spots, scotomas, etc., are all mapped at this distance. In the designing of the perimeter

[5]Furthermore it was reported by the observers employed in these experiments that when an after-image of threshold intensity for a distant projection had disappeared, it could be made to reappear by swinging in a near projection field.

The greater part of the determinations of the effect of distance on the chromatic response, positive and negative, were made under our direction by Dr. Lorle I. Stecher, who was at that time a graduate student in our department.

we have selected the distance which from all points of view we have considered the most feasible and satisfactory for practical work. Also we have devised methods which give much greater diagnostic sensitivity for this distance than was formerly obtained by working at greater distances than we have used, and in addition greater reproducibility of results and a better control of the conditions of the examination.

It seems to us unfortunate as well as unnecessary that Dr. Freeman should have obtained his values for the threshold in terms of size of stimulus. So little is known definitely of the effect of size of stimulus on the intensity of sensation for the different parts of the field even when the distance is held constant, that it is difficult to tell what or how much his results really mean. As the matter now stands the effect of distance on his results for the different parts of the field is inextricably confused with the effect of size. Dr. Freeman states that this method is hitherto unused in the measurement of sensitivity. In this connection it may be noted that size has been used as a measure but the result has been customarily called acuity. Dr. Freeman's results would ordinarily be classed as acuity results obtained at low intensities of illumination with a dark-adapted eye. In the determination of acuity when it is wished to minimize the effect of refraction or clearness of imagery and test as dominantly as possible the space sensitivity of the retina, it is customary to give the test object as little detail as possible; and when it is wished to emphasize the effect of difference in the resolving power of the refracting media, in other words to test for errors in refraction, the test object is given greater complication of detail. Dr. Freeman's test would rank therefore as an acuity test in which an effort has been made to minimize the factor of refraction. In testing the light sense the custom is to select a test object simple as to form or detail, hold the size constant and vary and measure the intensity. That is, the factor which exerts the greatest and most regular effect in the group of factors involved, in this case intensity of stimulus light, is usually selected as the one to vary and use as a measure. This procedure seems to us to be logical.

We have purposely confined our comments in this note to central vision. We feel that so little is known about the operation of various factors in peripheral vision, *e.g.* defects in imagery, accommodation, pupil reactions, etc., that discussion at this time would be academic if not futile. Many points must be cleared up before the study of vision in the peripheral field can be extended to the third dimension in accordance with a satisfactorily orderly and systematic plan.

It has not been our purpose to give critical comments on either the experimental work or the results reported in Dr. Freeman's paper. A few points, however, may be noted casually. (1) No special precautions seem to have been taken to control fixation for the determination of the threshold at the center of the field. The adequate control of fixation for such determinations is one of the most difficult points of technique in the determinations of the achromatic threshold in the dark room, and the lack of adequate control is one of the most important and frequent sources of variable and erratic results. Inasmuch as the sensitivity at the center was used in his study as unity for comparing the sensitivity at other points in the field, care in its determination was of basic importance. (2) In estimating the effective size of his stimulus in the peripheral field, Dr. Freeman seems to have ignored completely the tangent effect. This would introduce an error in his comparisons of sensitivity which would increase in magnitude as the

center is receded from. (3) He finds that achromatic sensitivity decreases quite regularly from fovea to periphery. He claims that this is the result usually obtained. This claim is contrary to fact. Achromatic sensitivity is depressed at the fovea when the eye is dark-adapted, as was the condition in his experiments. It increases for a distance towards the periphery and then decreases. At the extreme periphery it falls off quite sharply. His results conform more closely to the results which are ordinarily obtained for acuity in the peripheral field than they do to those on achromatic sensitivity, *i.e.* in acuity there is a continual decrease, sharp at first, from the fovea to the limit of the field. This accords with our comment above that his method of measurement is one that is ordinarily used in tests for acuity. (4) His device for control of fixation for the peripheral determinations does not seem designed to afford a sufficiently precise control of accommodation for a series of experiments whose object is the determination of the effect of distance.

Further, it has not been our purpose in the above presentations to offer any results or any analysis or statement of factors which might be construed as having any bearing whatever on the explanation of the anomalies obtained by Dr. Freeman. In case of anomaly, the obvious first need is further investigation. We have not thus far in our experiments obtained any results that can be considered as anomalous.

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Learning Curves for Poetry

In conducting some learning experiments recently, the writer obtained some evidence that curves for memorization of poetry are s-shaped. In order to test this phenomenon some specific experiments have been carried out in learning poetry. In the first experiment 3 poems of 20 lines each were used. The poems had 4 lines to the stanza and 4 feet to the line. Poems of 20 lines were chosen because previous experiments had shown that most of the *Ss* could be expected to memorize a poem of this length and type at one sitting of an hour or less.

Thirty *Ss* were used. Every one of the 3 poems selected was learned by 10 *Ss*. Since the poems proved to be practically equal in difficulty the results are treated collectively. The average number of trials required to learn these poems was 17.6. The *Ss* were instructed to read the poem aloud and to recite, after each reading, as much of it as they could. This process was continued until the poem was recited without error. The unit of progress used in this experiment was lines correctly recited. To obtain the learning curves, presented below, lines reproduced were plotted against trials.

The *Ss* were divided into 2 equal groups on the basis of their scores. Curve C, Fig. 1, represents the progress of learning on the part of the 15 best learners, and Curve D represents that of the 15 poorest learners.

Inasmuch as these poems proved to be rather difficult, it seemed advisable to plot learning curves also for easier poems. Several poems were tried. *Sir Patric Spens*, a ballad poem, proved to be one of the easiest, as determined by the scores obtained, so the first 20 lines of it were used. Sixteen of the *Ss* who had partici-

pated in the first experiment learned this selection. About an equal number of the poorer and better learners were chosen. The average number of trials required to learn this poem was 10.2. The same procedure was followed in treating the results in this experiment as in the preceding one. Curve A, Fig. 1, represents the learning progress for the 8 best learners and Curve B that for the 8 poorest.

It will be observed that all of these curves are s-shaped. It does not seem possible to fit the more usual negatively accelerated curve to the data. When the

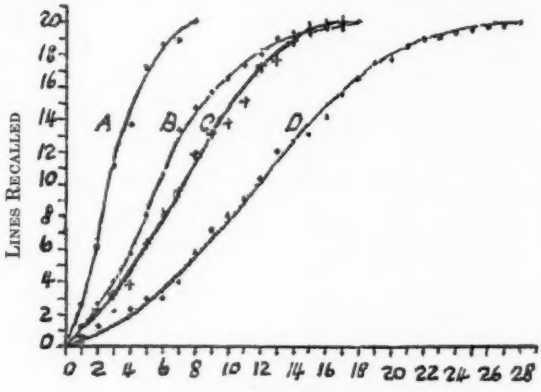

Fig. 1. Learning Curves for Poetry

A and C, curves of best learners; B and D, curves of poorest learners; A and B curves of easy selections; C and D curves of difficult selections.

TABLE I

Percentage of Lines Correctly Recalled during every Quartile of the Trials

Poem	Curve	Quartile			
		1	2	3	4
Difficult	C	23.5	36	25.5	15
Difficult	D	19	38	34.5	8.5
Easy	A	31	37.5	25	6.5
Easy	B	35	43.5	17.25	4.25

trials are divided roughly into quartiles, as in Table I, it may be seen that there is a higher percentage of recall, in the case of Curves C and D, in the second and third quartiles than in either the first or last; and that in the case of Curves A and B a greater percentage is recalled in the second quartile than in any of the others.

The interpretation of these findings is not easy. It may be that the size of the unit of response, a whole line, was conducive to learning curves of this kind. Con-

siderable learning may have taken place before the *S*s were able to reproduce many whole lines. This would necessitate several initial readings with relatively little reproduction. Furthermore, these results may be due to something inherent in the processes of learning material of this kind.

It seems justifiable to conclude that, within the limits of these experiments, learning curves for poetry are s-shaped. Final interpretations of this phenomenon must await further experimentation.

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THE 'CIGARETTE ILLUSION:' A REPLY TO MR. NEWMAN

Mr. Newman,[1] in his reply to my criticism[2] of Dr. Helson's 'cigarette illusion,'[3] holds that I have misstated and misinterpreted facts, and that I have "failed to meet Helson's objections to an explanation by contrast." In the following, I shall answer each of Newman's nine points, showing incidentally that the 'illusion theory' is unnecessary.

First, however, let us consider the concept of contrast itself. I quite agree with my critic that contrast, as he has defined it,[4] is inadequate to the phenomenon in question. The conception of simultaneous contrast which I have presupposed throughout this discussion is far broader than Mr. Newman's. Every color in the visual field tends to alter its surroundings in the direction of the color complementary to it. The colors may represent different surfaces seen under the same illumination, or different illuminations seen against surfaces.[5] This definition agrees essentially with the contrast concepts of such authorities as Hering, Helmholtz, Tschermak, and Kirschmann;[6] and, I believe, with current American usage as well.[7]

The inadequacies which Newman finds in the contrast theory of the cigarette phenomenon are of his own creation, for he has chosen to define contrast in a

[1]E. B. Newman, 'The cigarette illusion:' a reply to Dr. Wells, this JOURNAL, 43, 1931, 506-508.

[2]E. F. Wells, Dr. Helson's 'cigarette illusion,' this JOURNAL, 43, 1931, 136-139.

[3]Helson reported that the glowing tip of a cigarette looks green or yellowish-green when viewed under red illumination. (H. Helson, A new visual phenomenon—the cigarette illusion, *Psychol. Rev.*, 37, 1930, 273-275.)

[4]Newman, 506.

[5]The objection may be raised that inter-effects of different illuminations involve 'transformation' rather than contrast. But, at present, the evidence for an essential difference between contrast and 'transformation' is inconclusive.

[6]E. Hering, *Grundzüge d. Lehre v. Lichtsinn*, 1920, 115-141, 159-210; H. Helmholtz, *Treatise on Physiol. Optics*, trans. from the 3rd. German ed. by J. P. C. Southall, 1924, Vol. II., 264-297; A. Tschermak, Ueber Kontrast und Irradiation, *Ergebnisse d. Physiologie*, Vol. II., 2, 1903, 726-745; A. Kirschmann, Psychologische Optik, in E. Abderhalden's *Handb. d. biolog. Arbeitsmethoden*, 6, 1927, 959-962.

[7]Baldwin's *Dictionary of Philosophy and Psychology* defines simultaneous color contrast as, " The mutual effects, in respect to colour and brightness, which simultaneously seen but separated visual areas have upon one another." (Vol. I., 1901, 228.) In H. B. English's more modern *Student's Dictionary of Psychological Terms* (1928), color contrast is defined as the "fact that the juxtaposition of two colors results in a change in both such that each looks as if there had been mixed with it a little of the color antagonistic to the other."

narrow and unusual way, as "the induction by a given stimulus of the opposite effect in the adjacent unstimulated area." If literally interpreted, this definition has nothing to do with contrast. In order to obtain simultaneous contrast effects, one must have, not *a stimulus*, but two or more *stimuli*, qualitatively different. Furthermore, the area surrounding the so-called 'stimulus' is not *unstimulated*, but *differently stimulated*. If the surrounding area were actually unstimulated (absolute black in the physical sense), one would get, not contrast, but irradiation. The expression, "opposite effect," moreover, is meaningful only in terms of one specific theory of vision, that of Hering. This imposes an unfortunate limitation upon the definition. Even though we make allowance for these verbal errors, and read into the statement its apparent meaning—'the induction by a given color of its complementary quality in an adjacent neutral (grey or white) area'—it is still unsatisfactory for two reasons. First, it implies that contrast is an entirely one-sided process, whereas in reality it is reciprocal. Secondly, it excludes such well-recognized contrast phenomena as the inter-effects of adjacent chromatic colors. Since Newman has excluded the cigarette phenomenon from the category of contrast by definition, his argument against the contrast theory is gratuitous. He might better have devoted his energy to the defence of his definition.

(1) Newman's first specific objection is that I did not apply Helson's pinhole test properly, and consequently allowed a background of red to reach the eye. As a matter of fact, I did not use Helson's pinhole test at all. The test which I did use is described in my previous note.[8] If, however, I was unfair to Helson in describing the method by which I had seen the 'illusion' demonstrated to students and colleagues, *i.e.*, by viewing the glowing tip of the cigarette through a pinhole in a white card (whereas, Newman informs us, Helson used a *black* card in his test), I sincerely regret that the discrepancy between Helson's methods of testing and of demonstrating led to false implications.[9] Actually, of course, it makes no essential difference whether the card used in the pinhole test was black or white. In either case, red light is reflected into the eye of the observer from the card as well as its surroundings. Naturally the 'illusion' would persist, if viewed through a pinhole in a black card, for contrast would *not* be ruled out.

(2) As for Newman's second point, the disagreement probably hinges upon the use of terms. If Helson means that the energy curve for the cigarette light is higher in the red-yellow region than in the green-blue region, when he says, "more red and yellow are . . . . . emitted from the burning cigarette than green and blue,"[10] I have no quarrel with him on this score. The amount of any given color quality, however, is a function, not of the height of the curve alone, but of the area under the curve, and of the energy limen of the hue in question.[11] My

[8]Wells, 138.

[9]Newman's quotation from my note is misleading due to the omission of the middle of my sentence.

[10]Helson, 274.

[11]Newman's suggestion that the energy distribution of the cigarette glow may be obtained by use of a black body radiator merely begs the question of the possibility of selective emission, which is raised by Helson's statement, "It (the cigarette phenomenon) would thus appear to be a peculiarity of the tobacco. . . . ." (*Op. cit.*, 274.) The assumption that an unknown source would give the same energy distribution as a black body radiator of the same temperature is without justification.

statement, "spectroscopic analysis of the cigars and cigarettes showed green to be the predominant component,"[12] refers to psychological mixing value, not physical energy.[13]

Thus, the 'error,' which Newman believes biases my subsequent discussion, turns out to be merely an error of interpretation on his part.

(3) Thirdly, Newman objects to my 'explanation' of the "anomalies of the cigarette illusion" in terms of the physical composition of the lights. "What has the physical composition to do with contrast?"[4] This question is answered by facts previously reported.[14] The contrast color is determined by the physical composition of the lights just as their hue, tint, and chroma are so determined. But a more detailed explanation is apparently needed to satisfy my critic. According to the laws of color mixture, the composite effect of a lamp which shows a "full complement of spectral colors from red to blue"[15] will be a less saturated color than that produced by a lamp which shows practically only one hue.[16] The contrast colors of these relatively 'impure' lights were, consequently, distinctly lower in saturation than the contrast colors of relatively 'pure' lights. These contrast colors of low saturation, mixed with the light of the cigarette, would obviously produce less marked effects than would a highly saturated contrast color.

Had Newman stopped to consider these consequences of the physical composition of the lights, he could hardly have made the statement, "As far as the laws of contrast go, a red light is red and a yellow light is yellow regardless of their physical properties." There are many reds and many yellows. The casual observer, untrained in color work, frequently fails to distinguish between a 'pure' red and a slightly bluish-red, for example, while moderate differences in saturation commonly pass unnoticed. Even a trained observer finds these discriminations difficult in the absence of a standard of comparison.[17] Hence I chose to describe the lights in physical terms.

To be liberal, however, let us assume that Newman's statement, "a red light is red," etc., means that, if two light sources of different physical composition give red light identical in hue, tint, and chroma, then the contrast colors of the two lights will be identical. Will they? Not necessarily. It is unfortunate that Newman chose red for one of his examples, since the D.L. for hue is relatively large at the red end of the spectrum, especially beyond 636 mμ, whereas, in the complementary blue-green region, the D.L. is considerably smaller.[18] It is quite

[12]Wells, 136.

[13]Newman's inference that I based my conclusion upon the width of the spectroscopic band alone is incorrect. Nevertheless, the width of the band is one determining factor in so far as it indicates the wave-lengths transmitted.

[14]Wells, 136-137. [15]*Ibid.*, 137.

[16]As a matter of fact, the lamps in question did not give pure blue, green, and yellow illuminations respectively, but a slightly reddish blue, bluish-green, and greenish-yellow—all of noticeably lower saturation than the relatively pure red of the red lamp.

[17]It is more difficult to compare illuminations than object colors. (H. Krueger, Ueber d. Unterschiedsempfindlichkeit f. Beleuchtungseindrücke, *Zsch. f. Psychol.*, 96, 1924, 58-67.)

[18]O. Steindler, Die Farbenempfindlichkeit d. normalen u. farbenblinden Auges, *Sitz. d. Wiener Akad.*, 115, ii a, 1906, 39-62, *esp.* 48-49. For citation of other research on this subject, see J. H. Parsons, *An Introduction to the Study of Colour Vision*, 2nd. ed., 1924, 33-34.

possible to select two reds of different wave-lengths, psychologically identical, the complementaries of which are psychologically as well as physically different.[19] I cannot, therefore, agree with Newman's implication that the physical composition of light has nothing intrinsically to do with contrast.

Newman's concluding statement in this paragraph involves a logical error. Description of the conditions under which a phenomenon occurs is a valid part of scientific procedure. If the conditions are physical in nature, they must be described in physical terms; if chemical, in chemical terms, etc. But such description does not commit one to a physical or chemical theory of the phenomenon. By resorting to the physical properties of the stimulus, I have merely shown that the phenomenon in question is consistent with a contrast theory.

(4) The fourth point, like the third, rests upon Newman's failure to recognize that the physical composition of light is an important condition of contrast. I used the term "impurity of the lights," moreover, only in reference to the blue, green, and yellow lamps. Precisely how this impurity of the lights lessened the contrast effect, I have explained already. I neither used the term 'impurity' in referring to the cigarette, nor did I say that "the large amount of green accounts for the heightened effect." These are misstatements. The so-called "heightened effect" under red illumination is accounted for both by the relative purity of the red light and by the relatively large amount of green in the cigarette glow—not by its 'impurity' as such.

(5) When the statement quoted by Newman is taken in connection with the description of the experiments which it presupposes, it is clear that adaptation and negative after-effects are ruled out. The contrast effect occurs the moment that the colored illumination is turned on and persists without noticeable change throughout an observation period of several minutes.

I am accused, moreover, of slipping into "the logical error of using the description as an explanation."[20] Where in my note, may I ask, does Newman find a claim that the contrast theory constitutes an *explanation* of the phenomenon? Contrast is a descriptive category—not explanatory. The laws of contrast are descriptive laws. To be sure, contrast itself calls for 'explanation,' but that is a problem beyond the scope of the present paper. I have merely tried to show that the so-called 'cigarette illusion' belongs to a well-known category of events.

(6) My critic's sixth objection is that I have violated "the essential conditions of the illusion" because, in my critical experiment, I eliminated the conditions of contrast. Here again, Newman conveys a wrong impression by quoting a portion of a sentence, "rigid elimination of reflected red light," concluding, upon this basis, that the observer could not tell whether red light was present. Had he taken account of the phrase immediately following the one quoted—"*in the vicinity of the glowing tip*"—he might have avoided this error. My description of the set-up for the experiment indicates quite clearly that the observer was able to see the color of the illumination, since his range of vision naturally extended far beyond the small hole in the front of the black box. Reflected red light was thus eliminated in the vicinity of the glowing tip, but not from the total

[19]For recent determinations of complementary spectral colors, see W. Nagel's note in Helmholtz's *Optics*, *op. cit.*, 127-128.

[20]Newman, 507.

field, the outer walls of the box itself as well as the walls and floor of the dark room reflecting red light.[21]

Newman says that, without the customary contrast conditions, the effect will not appear.[20] In this he disagrees with Helson, who gave as one of his reasons for rejection of the contrast theory the alleged fact that "when no background of red is allowed to reach the eye, the end of the cigarette alone being visible, we still get the green effect."[22] In view of this definite statement, one would hardly infer from Helson's original discussion that the effect disappears with the removal of contrast conditions.

(7) Newman's experiment with the colored filters merits careful attention. An orange color, which was judged to be "equivalent to the cigarette tip," was produced by superimposing two colored filters in front of an electric light.[20] Under red illumination, this color "became only slightly more yellowish and did not approach green at all. . . . whereas the cigarette turned green under red light." Hence "an explanation based upon contrast is clearly inadequate."

The laws of contrast describe psychological events which occur when certain conditions are fulfilled. If these conditions are taken into account, it is obvious that the contrast effect should be greater with the cigarette tip than with the filters. Newman reports that the orange color produced by the filters was equivalent to the cigarette tip, meaning, I suppose, equivalent in hue, brightness, and saturation. Although this is at variance with my own observations we may pass this point, since, in other important respects, the two colors are not equivalent. First, the surface area of the filters is larger than the surface area of the cigarette tip. This alone might lessen the contrast effect considerably.[23] Secondly, the filtered color is not equivalent to the cigarette glow in psychological texture. It is a somewhat 'hard' luminous color, with definite front limits, which give it a surface-like character. The cigarette glow, on the other hand, is a 'soft,' filmy-bulky, luminous color, lacking definite front and back limits. Since the filtered color is much less film-like than the cigarette glow, the contrast effect is correspondingly decreased.[24] Lastly, under red illumination, considerable red light is reflected from the surface of the filters, tending to negate the contrast effect. When these differences in conditions are taken into account, it is not surprising that the effect is less pronounced with the filtered color than with the cigarette glow. In fact, the results of Newman's experiment might have been predicted from the contrast theory itself.[25]

If one agrees with Jaensch and his followers that inter-effects of different illuminations are 'transformation' phenomena rather than contrast, then one might suppose *a priori* that the cigarette phenomenon were a case of 'transformation,' and, therefore, "more than contrast." But this hypothesis is easily dis-

[21]Although the observer could see the red light, contrast was practically ruled out, since only colors in fairly close proximity induce a noticeable contrast effect.

[22]Helson, 275.

[23]Helmholtz, *op. cit.*, 267; D. Katz, *Die Erscheinungsweisen der Farben*, 1911, 631; G. Marzynski, Studien z. zentralen Transformation d. Farben, *Zsch. f. Psychol.*, 87, 1921, 47-51.

[24]Katz, *op. cit.*, 264-268.

[25]The yellowing of the filtered color represents a change in the direction of the complementary blue-green just as does the greening of the cigarette.

proved. If the cigarette tip, or any similar light source, is viewed by indirect vision, the contrast effect is markedly increased. Indirect vision *decreases* 'transformation' effects.[26]

(8) In his eighth point, my critic is obviously confusing the issue. Both Helson and the author found that the cigarette phenomenon occurs when successive contrast is ruled out.[27] But neither of us claimed that the effects which we obtained by simultaneous contrast might not be produced by successive contrast as well. The context of my statement, "red adaptation plays no noticeable part in the effect," makes clear that it refers to adaptation itself, not to its after-effects.

(9) If one assumes, as Newman does, that red light is red no matter what its physical properties, the results of the experiment with the two red lamps are difficult to understand. The illuminations of both lamps looked red. My observers called them red. But all 'red' is not red. Spectral analysis of the smaller lamp showed that it emitted orange and yellow light as well as red. It was an orange-red. Mixing contrast-aroused blue-green with the orange-red light might produce orange, yellow, yellow-green, green, or even blue-green, depending upon the proportions of the components.[28]

Thus my alleged misstatements and misinterpretations of fact turn out to be the fiction of my critic's misunderstandings. Newman's own position, on the other hand, is ambiguous. By implication, he agrees with Helson that the cigarette phenomenon is an illusion, but he has failed to demonstrate any illusory characteristics. His statement, that the phenomenon persists when contrast is ruled out,[29] suggests an illusion theory, but, on the next page, he admits "the fact that without the customary contrast conditions the effect will not appear."[30] Newman has failed either to present any cogent arguments against the contrast theory, or clearly to define his own position.

Smith College E. Frances Wells

The Cigarette 'Illusion:' A Case of Color Transformation

In her first note[1] regarding the change from an orange-red which the glowing end of a cigarette or cigar has in daylight or dark vision to a yellowish-green appearance under red illumination, Dr. Wells chose to defend the contrast theory which I had rejected,[2] among others, as an adequate description or explanation of the phenomenon. She asserted that the physical composition of the lights in question with the usual contrast effects would suffice to explain the direction of the shift in hue, basing her statements on spectroscopic analysis [*sic*] although

[26] Katz, *op. cit.*, 282-290; E. R. Jaensch and E. A. Müller, Ueber die Wahrnehmung farbloser Helligkeiten u. d. Helligkeitskontrast, *Zsch. f. Psychol.*, 83, 1919-1920, 328-330.

[27] "We see as good a green the moment the red light is turned on as after many minutes of exposure to red." (Helson, 275.) "The color of the glowing tip is the same at the end of a 10-minute period of adaptation as at the beginning." (Wells, 138.)

[28] This may be demonstrated by reference to the color-circle.

[29] Newman, 506. [30] *Ibid.*, 507.

[1] E. F. Wells, Dr. Helson's 'cigarette illusion,' this Journal, 43, 1931, 136–139.

[2] H. Helson, A new visual phenomenon—the cigarette illusion, *Psychol. Rev.*, 37, 1930, 273–275.

it is a well-known fact that spectroscopic analysis can tell next to nothing of the excitation values in a non-homogeneous source. She asserted that the predominant component in the cigarette glow is green "extending over half the visible spectrum" when her eye, unaided by the spectroscope, would have told her correctly that the dominant component must be in the red end of the spectrum. She erroneously reported that "in every case the contrasting color tends to appear" under slightly different conditions when still further variation in conditions would have shown her that the shift is often away from the contrasting color, as I reported in my original note. She asserts that the conditions for contrast were almost perfect for contrast, when, as a matter of fact the effect was obtained under conditions highly unfavorable to contrast.[3] She agreed that the greater the differences in intensity between the two lights (the cigarette tip and the illuminating source) the better the effect, although this is absolutely contrary to what would be expected from contrast mechanisms.

In her present communication on this subject, answering Newman's criticisms of her first one, she defines contrast loosely, leaving out of account at least two of the most important conditions for contrast and makes it include color transformations seen under different illuminations (of which the cigarette phenomenon is a special case) in spite of the fact that every writer on the subject since Katz's classical work of 1911 has been careful to make clear that these two sets of phenomena are not identical, no matter how closely related they may be.[4] She still holds to her erroneous view that green represents the predominant component in the spectrum of the cigarette glow, only changing her original statement to mean "psychological mixing" value even though this assertion has less to back it up than the original since it is assumed purely in order to get enough green from the cigarette to account for the green seen under the changed illumination. She still believes that looking through a spectroscope is sufficient to acquaint her with the amount of various wave-lengths present in a non-homogeneous light source and the excitation values of these wave-lengths when visual science teaches that nothing short of spectrophotometric analysis or its equivalent used in conjunction with some set of primary excitation values for brilliance and chroma can do this. She makes statements regarding the production of complementaries that are meaningless as they stand or false in the light of established

[3] When the eye was shielded from the red light, cutting its effectiveness far below that required for contrast.

[4] I can give but one or two quotations bearing out this statement, although the literature on transformation shows plainly enough that regardless of the relation assumed by different writers to exist between the two, contrast and transformation are different processes. For example, "Solche Transformation eine eigenartige, auf gewöhnliche Kontrast und farbige Umstimmung des Auges nicht zurückführbare Erscheinung ist," S. W. Kravkov and W. A. Paulsen-Baschmakowa, Über die kontrasterregende Wirkung der transformierten Farben, *Psychol. Forsch.*, 12, 1929, 88; D. Katz has expressed a similar view in his *Erscheinungsweisen der Farben*, 1911, 270; E. R. Jaensch agrees in his numerous articles on the subject of which see: Über den Farbenkontrast und die sogennante Berücksichtigung der farbigen Beleuchtung, *Zsch. f. Sinnesphysiol.*, 52, 1921, 166; G. E. Müller while disagreeing with Jaensch that they are even parallel phenomena asserts that they are quite distinct in his article entitled, Über Jaensch's Zurückführung des Simultankontrastes auf zentrale Transformation, *Zsch. f. Psychol.*, 93, 1923, 1–16.

facts regarding the system of complementary wave-lengths. She rightly sees that physical composition is important in determining purity, saturation and hue of seen colors but errs in assuming it is important in determining contrast colors after the average or net effect of the composite stimulus is known in vision. She too easily disposes of the possibility that the phenomenon is a transformation phenomenon on the basis of a single criterion which depends upon another condition far more important than the one she mentions. She accuses Newman and me of holding to an illusion theory of the phenomenon when I explicitly stated that the seat of the effect is probably in the eye.[5]

On the other hand Dr. Wells fails to take any notice whatsoever of the experiments with disks which I reported in my note wherein conditions for contrast were fulfilled yet no change analogous to the change in the hue of the cigarette was observable. She also fails to appreciate the fact that if the cigarette is viewed under blue illumination it becomes not yellower, as would be expected on the basis of her contrast-mixture theory, but actually redder.[6]

It will not be difficult to show that the contrast theory fails to account for all of the facts when a wider range of illuminations is used and a slightly better stimulus (the filament of a carbon lamp) replaces the cigarette.

Let us proceed with the contrast theory as handled by Dr. Wells. All writers on contrast regard as essential conditions for the phenomenon that *neighboring* fields shall be differently stimulated, speaking of *benachbarter Elemente, Infeld,* and *Umfeld,* and in other like terms denoting adjacent areas.[7] This condition or assumption is most important for my pinhole experiment which I have repeated under better conditions using a blackened tube which admitted light through a small aperture just sufficient to include the tip of the cigarette. In my original experiments I was sure that *surrounding* red light was definitely excluded but

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[5]Had Dr. Wells designated our use of the word 'illusion' as improper for the phenomenon in question she would have been quite right. But she can hardly accuse me of holding an illusion theory of the phenomenon in view of my concluding paragraph which she must have read: "That this is not a purely subjective phenomenon can hardly be doubted by anyone who takes the trouble to observe the effect for himself. A striking exception to our statement that all observers see the glow as green furnishes strong argument for the peripheral origin of our phenomenon: in a group of persons where we tried to demonstrate this illusion under very unfavorable conditions the only one who failed to see any green at all in the glow was a deuteranope! This seems to point to the eye as the seat of the effect" (Helson, *op. cit.,* 275). Since when have activities in the eye itself been regarded as 'illusions?' The word illusion was used rather in the sense of an unexpected phenomenon.

[6]This was implied in my original note and has since been established by further experimentation to be described in this note.

[7]Even the very authors quoted by Dr. Wells make this an essential condition for contrast. Thus, to quote only one, A. Tschermak says: "Simultaneous contrast means that a given colored excitation causes a secondary, complementary excitation in neighboring elements," Über Kontrast und Irradiation, *Ergebnisse der Physiol.,* 11, 2, 1903, 727 and again on 736 where he quotes Hering to this effect. Jaensch lays down as the essential condition of contrast the *Umfeld* while in transformation the *Umfeld* is replaced by a *beleuchteter Raum,* a fact which places the cigarette phenomenon in the latter class of phenomena as we shall see later.

acting on the results reported by Cramer[8] I made special effort to discover if the medium between the cigarette and the eye were illuminated with red. I could barely distinguish a faint reddish haze caused by reflection and other causes at the edges of the aperture and elsewhere in the field. It has been established by Cramer that if the medium between the eye and the colored object is illuminated conditions are favorable for transformation effects, amount of transformation being *inversely* proportional to the visibility of the illumination. No wonder I missed it in my original experiments since I was looking, in accordance with the conditions for contrast, for an *Umfeld* of red. There is no question here of simultaneous stimulation of adjacent retinal areas since the illumination of the *Zwischenmedium* must affect the same retinal area that the cigarette does. Since the amount of transformation is inversely proportional to the visibility of the intervening medium, the very difficulty in observing it is proof that this is a case of transformation and not of contrast.

Since the amount of contrast-induced color varies with the amount of stimulation (a logarithmic relation according to Kirschmann)[9] the change in the color of the glow should have been markedly reduced when looking through the pinhole of the tube since the amount of red light falling upon the eye is reduced by at least 95%; but there is little, if any, perceptible change in the green of the cigarette if some illumination overspreads the cigarette tip (in the eye, of course). In her original note Wells argued that the white cardboard used as a shield acted as an excellent reflector—indeed a most excellent reflector would be necessary if enough light were to reach the eyes with the shield to keep the cigarette at all green. As a matter of fact the evidence presented in my publication was based upon a dark piece of cardboard which certainly reduced the amount of light beyond any possibility of contrast mechanisms functioning. With the tube or shield the saturation of the green is altogether too great to be ascribed to contrast mechanisms set off by the slight amount of red present.[10]

What of Dr. Wells' easy disposal of the phenomenon as a transformation phenomenon because the evidence for a difference between contrast and transformation is "inconclusive" and because the effect is increased in peripheral vision whereas it should be decreased if it belongs to the category of transformation colors? In the first place Dr. Wells' inclusion of "different surfaces seen under the same illumination, or different illuminations seen against surfaces" under contrast is absolutely opposed to all writers on the subject who, whatever their disagreements, are unanimous that such phenomena are distinct from contrast and should be kept separate.[11] Dr. Wells betrayed too much haste in

[8]Thea Cramer, Über die Beziehung des Zwischenmediums zu den Transformation– und Kontrasterscheinungen, *Zsch. f. Sinnesphysiol.*, 54, 1922, 215–242: "The more the intervening medium approaches a clear, colorless character, the greater the transformation effect" (215).

[9]A. Kirschmann, Psychologische Optik in E. Aberhalden's *Handb. d. biolog. Arbeitsmethoden*, 6, 1927, 959 ff.

[10]Katz and Jaensch both make this same point, Katz, *op. cit.*, 270; and Jaensch, Über Grundfragen der Farbenpsychologie, *Ber. d. VI Kongress f. exp. Psychol.*, 1914, 45–46.

[11]It is hardly necessary to say that not all writers are agreed on all points regarding color transformation but on the fact that it is not to be identified or confused with contrast there is almost perfect agreement. There are disagreements concerning the relation between contrast and transformation, the locus of each in the nervous system, the direction of transformation, etc., but these do not affect my statement of agreement on the one point.

disposing of the effect as a transformation effect merely on the single criterion of its goodness in peripheral vision. She has neglected to note that if the intervening medium remains equally or less visible in peripheral vision, the effect will be as good or better in peripheral vision. It is a well-known fact that red is best seen, of all colors, in the fovea, and hence the intervening medium would be most pronounced in foveal vision, making the transformation effect, in accordance with Cramer's findings quoted above, poorer there and better in indirect vision. Cramer reported that the reason transformation effects are increased in indirect vision was because the intervening medium became less noticeable.[12] Dr. Wells has also failed to note that there are, as Katz has pointed out,[13] huge individual differences with respect to transformation phenomena. I have not been able to verify her assertion that the effect is increased in indirect vision. Under blue illumination, however, the cigarette becomes *redder*, and the transformation decreases in indirect vision as I have been able to verify very easily. The whole subject is too complicated to be settled by reference to a single criterion, especially when it may be negated by others.

We are ready to return to the contrast theory as used by Dr. Wells. If the change in the cigarette is verily a contrast phenomenon how does it come about that a red inducing field can result in a beautiful green in the reacting field when the laws of contrast tell us that contrast effects are greatest (1) when inducing and reacting fields are farthest apart, *i.e.* are complementaries to begin with; and (2) when brightness differences are least. Helmholtz,[14] Kirschmann,[15] Troland[16] and others have pointed out that when similar hues are involved the net effect should be gray, or at best the same color at a low degree of saturation. Helmholtz says: "When a particular color is made dominant in the visual field a paler shade of the same hue will look white." It is needless to multiply quotations. Kirschmann even adds that contrast requires that adjacent retinal areas shall be *differently* stimulated.[17] Dr. Wells has reported that the effect is better when the two lights are of unequal intensity, something I have also found since lights of equal intensity have no effect upon each other at all. How does this fit with the law given by Helmholtz that "contrasting coloring is most positive

[12]Cramer says: "Bei seitlichen gehaltenem Kopf ist das Zwischenmedium deutlicher und die Transformation geringer. Alle diese Versuche ergaben also, dass die Transformation abnimmt, wenn die Deutlichkeit der Zwischenmediums steigt" (*op. cit.*, 226). But if for any reason the intervening medium became less clear in indirect vision, as in the case of red, then the transformation effect would increase since the factor of *beleuchteter Raum* is primary for transformation phenomena.

[13]D. Katz, *Ber. d. VI Kongress f. exp. Psychol.*, 1912, 187: [Transformed colors] "haben eine merkwürdige Labilität und weisen enorme individuelle Unterchiede auf."

[14]H. von Helmholtz, *Physiol. Optics*, trans. from the 3d German ed. by J. P. C. Southall, 1924, II, 274 ff. for the quotations which follow.

[15]A. Kirschmann, *op. cit.*, 967: "If you place a less saturated colored paper on a larger equally bright paper of the same hue, then the former appears gray."

[16]L. T. Troland, *Sensation*, 1930, p. 175: "If the test field already shows a blue, a yellow value in the surrounding field will bring about an increase in blue saturation; while a blue value in the surrounding field will decrease the saturation." *Mutatis mutandis* we should expect red with red to desaturate the test field or produce a gray at best if we begin with a red.

[17]Helmholtz, *loc. cit.*

where the inducing and reacting colors are equally bright, and their difference is not one of luminosity but simply of color."[18] Here we have no difference in hue and great difference in luminosity or brightness! We are asked to believe, by Dr. Wells, that a red *Umfeld* will make a red *Infeld* a good green—which is more than anyone has demanded of contrast mechanisms. We are asked further to believe that without a surrounding field, as in my tube observations, that the red illumination acts simultaneously to desaturate the red and produce a green process in one and the same retinal area!

Dr. Wells' piece of legerdemain with the contrast theory is brought about by an appeal to the supposedly predominant character of the green in the spectrum of the cigarette glow and, implicitly, a fatigue hypothesis. Dr. Wells' iterated and reiterated assertions that spectroscopic analysis enabled her to determine what wave-lengths were present or absent in the light sources and which possessed greatest psychological mixing value are based upon a faulty knowledge of the amount of information that can be gathered from spectroscopic analysis. Spectroscopic analysis is no safe guide to the *absence* of certain wave-lengths from the spectrum. Recently D. B. Judd and I have found it necessary to determine if the statements made by early writers regarding the transmission properties of certain filters could be true in the light of the reported results. Upon subjecting the filter in question to *spectrophotometric* analysis Judd wrote me that although the claim was made that it transmitted light from only the red end of the spectrum "it had a measurable transmission throughout the whole spectrum. Examination of this filter by means of a hand spectroscope failed to show the slightest trace of the green and blue light which is known to be transmitted. It would have been easy to conclude immediately that the filter only transmitted from the red end of the spectrum; and it is certain that the green and blue light could not have been detected by Helmholtz if he viewed the whole spectrum at once." What then becomes of Dr. Wells' naïve assertions regarding the predominance of the green, based as they are upon 'spectroscope analysis'? Determination of chromatic valence requires a much more complicated technique. Had she left the spectroscope alone she would have been nearer the truth because the normal color of the cigarette shows to the unaided eye that orange-red must be the strongest component. If the red of the cigarette did not possess the greatest psychological mixing value (whatever that phrase may mean!) the glow of the tip could not possibly appear orange-red for there is no other place for red to come from. We do know, however, that the cigarette tip and the carbon filament of an incandescent lamp (when seen as orange-red) are sufficiently like a black body radiator at a certain temperature to accept the emissivity of the latter as representative of the former.[19] This fact, coupled

[18] Kirschmann, *op. cit.*, 967.

[19] Dr. Wells' assertion that "the energy distribution of the cigarette glow may be obtained by the use of a black body radiator merely begs the question of the possibility of selective emission. The assumption that an unknown source would give the same energy distribution as a black body radiator of the same temperature is without foundation" is really beside the point since the question of physical composition has been seen to have lost its importance except as determining the seen color, after which the contrast color is given as the complementary. But for the sake of accuracy regarding physical composition let me quote from a

with other data which have been assembled by the Committee on Colorimetry for 1920–21, enables us to say that the color-excitation value of the radiation from the cigarette and the carbon filament is predominantly red, with green second, and violet negligible.[20]

Dr. Wells must assume that the contrast-aroused green (even with the tube when the amount of red reaching the eye is so faint as to be hardly discernible) possesses sufficient brilliance and saturation to completely obliterate the red excitation leaving the good green that is observed. Not even by 'mixing' contrast-aroused green with the green emitted from the cigarette would there be enough green to account for the green that appears. At this point Dr. Wells might have assumed that fatigue or adaptation reduces the red excitation sufficiently to allow the green excitation to appear in sensation. This theory is actually more plausible than Dr. Wells' but since she agrees with me that "red adaptation plays no noticeable part in the effect" she has lost the only prop which could give her theory the least verisimilitude.

Finally, with regard to contrast and physical composition, we must note that physical composition is of importance in contrast phenomena only in so far as it determines the hue and other dimensions of the color seen. At least this is in keeping with the usual views regarding contrast since no mention is made of anything but seen color. Once the hue, saturation, and brilliance of the seen color are given, which are what I have called the net or average effect of a non-homogeneous light source, the contrast color, according to the theory of contrast, follows at once as the complementary. Newman's statement that "as far as the laws of contrast go a red light is red and a yellow light is yellow regardless of their physical properties" is quite in keeping with a consistent theory of contrast. Dr. Wells' choice of green as the color to be mixed with the contrast color because of its greater psychological mixing value appears to be a purely

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communication by D. B. Judd from the colorimetry section of the Bureau of Standards on this point: "There is little reason to doubt that the radiating surfaces of the cigarette tip are incandescent carbon. If this be the case, the deviations of the spectral energy distribution of the emitted beam from that of a black-body radiator at some temperature are unimportant. For example, the energy distribution from a carbon filament lamp is customarily represented by Planck's formula. In so far as an appreciable portion of the light from the cigarette tip comes from cavities rather than from exposed surfaces, the approximation to an energy distribution which may be represented by Planck's formula should be even better than that yielded by the relatively straight filament of the carbon lamp." I might add that one can find the percentage excitation values for black-body colors in Table 17 of the Report of Committee on Colorimetry for 1920–21, by L. T. Troland (*J. O. S. A. and R. S. I.*, 6, 1922, 590) which will show that for the carbon filament when fairly red the red excitation value is in the neighborhood of three times that of the green, the violet being negligible, the same being true for the cigarette on the basis of the above discussion of its similarity to a black body radiator.

[20]The emissivity of the carbon filament is so close to that of a black body radiator that it furnishes an excellent source for transformation phenomena especially since its color can be so accurately controlled, whence its spectral distribution and color excitation values can be easily determined from published data.

ad hoc procedure justified neither by the theory of contrast nor by her so-called analysis of the light.[21]

The test of theory is experiment and more experiment. Acting upon a suggestion made by Judd, I used in place of the cigarette the filament of an ordinary (carbon) unfrosted lamp of 16 c.p. By reducing the current flowing through the lamp the color of the filament will be seen to change from a bright, almost white, yellow through orange having more yellow than red, then taking on more and more red until a deep dark red remains. Choosing an orange of about equal red and yellow valence I observed this light under blue illumination. If the contrast theory is correct, then the blue illumination should swing the orange toward yellow by inducing yellow. It doesn't. There is a decided shift toward the red and the filament becomes *redder*. In fact the current can be regulated so that the filament shows just enough yellow to be cancelled when the blue light is turned on and the filament becomes an almost pure red with no yellow present. The shift toward red under blue illumination appears with the cigarette also, but not so beautifully as in the case of the filament. If we take the same orange filament and observe it under yellow illumination that we did under blue, we shall find that it then appears redder, although from the contrast theory we would expect it to show some blue. Only in the case of red and green illumination does a shift toward the complementary color take place and this single fact apparently led Dr. Wells astray. Numerous other experiments which I have performed with differently colored objects and illuminations show that the shift is not toward the complementary color. This fact has already been reported upon in the literature in the case of colored papers.[22]

We come now to the reasons for ascribing this phenomenon to that class of color experiences known as color transformations. I shall have to give them briefly since any extended discussion would take us far beyond the limits of this note. (1) The cigarette and the filament which appear in one hue under normal

[21] I must point out that Dr. Wells' statement that "it is possible to select two reds of different wave-lengths, psychologically identical, the complementaries of which are psychologically as well as physically different" is contrary to well-established principles of color analysis and mixture data. In fact as the statement stands, following her statement regarding the limen for hue, it is meaningless. It happens to be true that wave-lengths greater than 660 mμ are not ordinarily distinguishable as different reds, a fact which may be what Dr. Wells has in mind. But the complementaries of these indistinguishable hues must all lie at one point on the opposite side of the color triangle since they are all referred to 660 mμ with which they are identical. Both the table of complementaries by Nagel, referred to by Dr. Wells, and the discussion of this important topic in the report of the colorimetry committee fail to substantiate her assertion that such complementaries are psychologically as well as physically different. Again, her discussion of composition of the sources is beside the point for she has nowhere made or given a description of the lights in physical terms nor has she referred to any measurements to substantiate her assumptions. The black-body radiator from 1000–2000 degrees Kelvin probably comes as close to furnishing a physical specification of the cigarette as can be had without resort to spectrophotometric analysis.

[22] It would appear that Jaensch's belief that the color of the transformed color always shifts toward the complementary hue was based upon a too limited assortment of illuminations, as several writers have pointed out. For evidence on this point see S. Krauss, Das Farbensehen in bunter Beleuchtung und die Farbenkonstanz der Sehdinge, *Zsch. f. Psychol.*, 100, 1926, 50–153.

illumination or absence of illumination, show a change in hue when illuminated by colored light. This satisfies the definition of a transformation color which has been variously defined as 'that color which appears as a result of a change in brightness or color when subjected to abnormal illumination.' (2) The conditions described in the literature as the conditions for transformation are fundamentally the same as those governing our observations of the cigarette and the filament: the light coming from the colored object must remain the same under different illuminations but is seen as different. The cigarette and lamp filament act to absorb all incident light (very clearly like a black body) and by the same token each emits its own spectrum regardless of the light falling on it. This is the first use of self-luminous bodies for transformation effects to my knowledge and allows a very simple means of demonstrating it without the necessity for special apparatus (Katz' and Jaensch's 'reduction technique'). (3) Contiguous retinal areas are not involved but there is an illumination of the intervening medium, as in the case of transformation colors. (4) The amount of illumination reaching the eye is not sufficient to account for the observed change in hue and saturation on the basis of contrast, a fact which has been pointed out in the literature. (5) The direction of change is not always toward the contrasting color of the illuminant. While there is some disagreement on the direction of change of transformed colors, it is certain that the shift need not always be toward the contrasting color. On the other hand, writers who have worked under conditions favoring the appearance of the contrasting color nevertheless deny that we are dealing with contrast mechanisms. My experiments are conclusive so far as the fact of non-complementary shifts is concerned. (6) The effect in the case of the filament under blue illumination is decreased in indirect vision and we have accounted for the increase in effect in peripheral vision in the case of red illumination by reference to the part played by the illumination, susceptibility of the fovea to red, and individual differences in transformation phenomena. (7) The effect does not subside with *Dauerbeobachtung* as would be expected if it were contrast. (8) The brightness difference in the two sources of light is so great as to rule out contrast which is best when brightness differences are least.[23]

The easy disposal by Dr. Wells of the cigarette phenomenon as merely a case of contrast harks back to the views of twenty years ago regarding such phenomena. While to Dr. Wells the evidence for a distinction between contrast and transformation colors is inconclusive, to such writers as Katz, Jaensch, Bühler, Granit, and G. E. Müller, there is sufficient difference between them to merit separate investigation and separation in the interests of truth, adequacy, and the furthering of knowledge about color vision. While to Dr. Wells this is merely something to be labelled contrast and dismissed (just as she hastily dismissed the possibility that all changes in hue might not be toward the complementary color), to others the change in hue which colored objects undergo under abnormal illumination involves problems extending to the farthermost limits of color psychology and physiological optics.

[23]One other test which should be made is this: the after-images of transformed colors show no change, I have tried this test but found difficulties in getting good after-images under the conditions of observing the cigarette and the filament. Neither stimulus is intense enough to give good after-images. I feel sure, however, it can be done.

Pouring new wine into old bottles may have some virtue when the flavor of the new wine corresponds to the labels on the old bottles. But when it doesn't it may require the services of a specialist to determine what the wine really is. All of the specialists whom I have been able to consult in the literature seem to agree that the label of contrast should not be affixed to the change in the color of objects observed under abnormal illumination, no matter how much they may disagree on various other questions raised on this comparatively new subject.

Bryn Mawr College HARRY HELSON

A REJOINDER

Dr. Helson now claims that the cigarette phenomenon, which he called an 'illusion' in his original report,[1] is a case of color 'transformation.'[2] He admits that the term 'illusion' was a misnomer, explaining that he used it in the sense of "an unexpected phenomenon." The cigarette 'illusion' is not an unexpected phenomenon, for the effects are predictable from the laws of contrast.[3]

(1) Dr. Helson ignores many facts and arguments previously presented in this discussion as well as much recent literature on contrast and 'transformation.' (2) He has made numerous misstatements. (3) Recent experimental work shows that the older definition of 'transformation' which Dr. Helson quotes is inadequate and misleading. (4) Dr. Helson's description of his experiments is so incomplete that it is impossible either to repeat the work or to evaluate the results. (5) Dr. Helson bases his interpretation of his results upon a single piece of work, done under entirely different conditions, which has been proven unsound by more recent experiments. (6) He assumes that surface colors are equivalent to luminous colors, although it has been recognized ever since 1911 that this is not so. (7) He takes it for granted that 'transformation' is something essentially different from contrast, ignoring negative evidence.

(1) Dr. Helson ignores previously reported facts when he says that I fail to appreciate his anomalous color changes. These changes were not found in my experiments.[4] He repeats Newman's charge that I naïvely assumed that spectroscopic ana'ysis provides an accurate physical description of the composition of light, although I have specifically stated that I made no pretense at such description.[5] He likewise ignores my twice-repeated argument that colored surfaces are not equivalent to luminous sources for purposes of contrast experiments.[6]

(2) Dr. Helson says that I took no notice of his experiment with gray disks.[7] Apparently he has overlooked my whole argument against the equivalence of colored surfaces and luminous sources as well as the footnote which refers specifi-

[1] H. Helson, A new visual phenomenon—the cigarette illusion, *Psychol. Rev.*, 37, 1930, 273-275.

[2] Helson, The cigarette 'illusion:' A case of color transformation, this JOURNAL, 43, 1931, 691-700.

[3] E. F. Wells, Dr. Helson's 'cigarette illusion,' *ibid.*, 43, 1931, 136-139. Dr. Helson's brief reference to the possibility of peripheral origin does not necessarily indicate that the effect is non-illusory. Peripheral factors in illusions have been recognized since 1904.

[4] *Ibid.*, 136-137.

[5] Wells, The 'cigarette illusion:' A reply to Mr. Newman, *ibid.*, 43, 1931, 686-691.

[6] Wells, *supra*, 137; 690.

[7] Helson, *supra*, 693.

cally to the disk experiment.[8] Again, he claims that his experiments represent the first use of self-luminous bodies for 'transformation' effects, whereas both Krauss and Bocksch have antedated him in this respect. In fact, Bocksch actually described Dr. Helson's 'cigarette illusion' in 1927.[9] Another example of misstatement is the assertion that the variation of "the amount of contrast induced color" with the "amount of stimulation" is a logarithmic function according to Kirschman.[10] Kirschmann's logarithmic law refers, not to the "amount of stimulation," but to the relation between the saturation of a colored *Umfeld* and the degree of contrast in a gray *Infeld*. Dr. Helson's citation, moreover, is incorrect.[11]

(3) Dr. Helson's definition of 'transformation' as dependent upon 'abnormal' illumination dates as far back as 1911—20 years ago! Both Köhler and Gelb have brought evidence against this older notion of 'transformation.' As Köhler pointed out in 1915, the distinction between 'normal' and 'abnormal' illumination, with its correlative distinction between 'retinal' (*eigentlichen*) and 'transformed' (or *berichtigten*) colors has lost all genetic meaning.[12] More recently, Gelb has shown, by a clever modification of Katz' and Jaensch's experiments, that the distinction between 'normal' and 'abnormal' illumination leads to absurdity.[13] The seeing of surface colors in 'normal' illumination, he concludes, is just as problematical as seeing them in 'abnormal' illumination.[14]

(4) Dr. Helson's description of his important experiment with the black tube is so inadequate that it is impossible to attach significance to the results. One would like to know what he means when he says, "the amount of red light falling upon the eye is reduced by at least 95%," and how he determined this figure. One would also like to know the position of the source of illumination and the technique employed to eliminate successive contrast and binocular contrast.

(5) Dr. Helson bases his interpretation of the results in the black tube experiment upon Cramer's work.[15] Not only were the conditions of Cramer's experiments entirely different from Dr. Helson's, but her interpretation of the results (which Dr. Helson accepts without reservation) is open to serious criticism. The assumption that the *Zwischenmedium per se* may affect 'transformation' phenomena rests upon Bühler's *Luftlicht* hypothesis. But Katz has proven unequivocally that this hypothesis is untenable, that the *Zwischenmedium per se* is

[8]Wells, *supra*, 137.

[9]H. Bocksch, Duplizitätstheorie und Farbenkonstanz, *Zsch. f. Psychol.*, 102, 1927, 228-449, *esp.* 390; S. Krauss, Tatsachen und Probleme zu einer psychologischen Beleuchtungslehre auf Grundlage der Phänomenologie, *Arch. f. d. ges. Psychol.*, 62, 1928, 179-224.

[10]Helson, *supra*, 694.

[11]A. Kirschmann, Ueber die quantitativen Verhältnisse des simultanen Helligkeits- und Farben-Contrastes, *Philos. Stud.*, 6, 1891, 417-491, *esp.* 491.

[12]W. Köhler, Optische Untersuchungen am Schimpansen und am Haushuhn, *Abh. d. kgl. Akad. d. Wiss. zu Berlin, phys.-math. Kl.*, 3, 1915, 1-70.

[13]A. Gelb, Die "Farbenkonstanz" der Sehdinge, *Handb. d. Normalen u. Patholog. Physiol.*, 12, 1929, 594-678, *esp.* 653-654, 668-669.

[14]*Ibid.*, 672.

[15]T. Cramer, Ueber die Beziehung des Zwischenmediums zu den Transformations- und Kontrasterscheinungen, *Zsch. f. Sinnesphysiol.*, 54, 1922, 215-242.

not an effective factor.[16] Bühler himself has implicitly recognized the significance of Katz' experiments.[17]

Dr. Helson's interpretation of Cramer, moreover, is erroneous. He says, "Cramer reported that the reason transformation effects are increased in indirect vision was because the intervening medium became less noticeable."[18] But what Cramer actually reported was that in indirect vision the *Zwischenmedium* becomes more distinct and 'transformation' is decreased.[19] Dr. Helson's argument that, under red illumination, the 'transformation' effect must be less in foveal vision than in peripheral vision because "red is best seen, of all colors, in the fovea,"[20] is contrary not only to his own previous findings and to Cramer's results, but to the established facts of differential sensitivity of the retina. Cramer did not find these supposedly unique effects with red illumination. She says specifically, "*Die Untersuchung mit rotem Licht führte zu ganz entsprechenden Resultaten*" (as with other illuminations).[21]

(6) Passing on to my sixth point, I need only cite the work of Katz and the extensive *Gestalt* literature to show that structural properties of the visual field are important factors in contrast and 'transformation' effects.[22] Surface colors are not equivalent to luminous colors. Hence one has no right to assume, as Dr. Helson does, that results obtained with one may be applied to the other.

In taking for granted that 'transformation' is essentially different from contrast, Dr. Helson seems to have ignored important negative evidence. True, the authors whom he cites refuse to identify the two categories, but at least three of these authorities have suggested that they may be not essentially different. Jaensch, for example, speaks of the *Wesenszusammenhang* of the two realms, of their structural and phenomenological similarities. He derives contrast effects genetically from prior 'transformation' experiences.[23] If there is a *Wesenszusammenhang*—a genetic relation—between two phenomena, are they essentially different? Bühler has suggested the possibility of a *Tiefenkontrast* which "*in der Tiefendimension benachbarter Schichten aufeinander gibt.*"[24] Granit, another of Dr. Helson's authorities, refers contrast and 'transformation' to the same mechanism.[25]

The more important arguments which have been advanced for the separation of contrast from 'transformation' are as follows.

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[16]D. Katz, Neue Beiträge zu den Erscheinungsweisen der Farben. Luftlicht und Beleuchtungseindruck, *Zsch. f. Psychol.*, 95, 1924, 129-136. See also Katz' review of Bühler's *Die Erscheinungsweisen der Farben*, *Psychol. Forschung*, 5, 1924, 171-182.

[17]K. Bühler, Gegenbemerkungen, *ibid.*, 182-188.

[18]Helson, *supra*, 695.

[19]Cramer, *op. cit.*, 226.

[20]Helson, *supra*, 695.

[21]Cramer, *op. cit.*, 223.

[22]Katz, *Die Erscheinungsweisen der Farben*, 1911. The *Gestalt* literature is too extensive to cite here. Gelb's treatment is representative (*op. cit.*).

[23]E. R. Jaensch, Ueber Grundfragen der Farbenpsychologie, *Ber. d. VI Kongr. f. exper. Psychol.*, 1914, 45-56, *esp.* 55; Ueber den Schluss von Gesetzesanalogien auf kausale Abhängigkeiten, *Zsch. f. Sinnesphysiol.*, 54, 1922, 251-254, *esp.* 254.

[24]K. Bühler, *Die Erscheinungsweisen der Farben*, 1922, 19.

[25]R. Granit, Farbentransformation und Farbenkontrast, *Skan. Arch. f. Physiol.*, 48, 1926, 147-224, *esp.* 217.

(*1*) *Transformation effects are much stronger than contrast effects.*[26] Since contrast effects vary widely with conditions, this difference *may* be due simply to different conditions in the two classes of experiments. A merely quantitative difference, moreover, does not prove a difference in kind.

(*2*) *There are large individual differences in transformation, but not in contrast.*[27] Large individual differences have also been reported in contrast.[28]

(*3*) *Transformation is eliminated by the adoption of a critical attitude but contrast is not.*[29] This argument is based upon the well-known fact that artists learn to eliminate 'transformation' by viewing objects under a critical attitude, but they still see contrast. This is true, however, only for *Randkontrast. Flächenkontrast*, like 'transformation,' may be similarly eliminated.[30]

(*4*) *Transformation occurs only with surface colors, whereas contrast occurs at its best with film colors.*[31] Gelb's experiments on patients with brain lesions provide an adequate answer to this argument.[32] The cigarette phenomenon itself is an instance of alleged 'transformation' in a non-surface color. 'Transformation' is not limited to surface colors, as Katz claimed.

(*5*) *Transformation effects are greater in direct vision than in indirect, while the opposite is true of contrast.*[33] According to Katz, colored shadows are contrast phenomena, and should, therefore, show up better in peripheral vision.[34] But Kroh says that colored shadows are 'transformation' effects.[35] If so, they should appear better in central vision. Only further experimental work can decide this point.

(*6*) *Objectivity and surface character increase transformation effects, but weaken contrast effects.*[36] This argument rests in part upon a failure to distinguish between objectivity and surface character. 'Transformation' effects with glows are greater than with surface colors. Objectivity, on the other hand, increases contrast as

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[26]Katz, *op. cit.*, 270; Jaensch, Ueber den Farbenkontrast und die sog. Berücksichtigung der farbiger Beleuchtung, *Zsch. f. Sinnesphysiol.*, 52, 1921, 165-180, *esp.* 166; O. Kroh, Ueber Farbenkonstanz und Farbentransformation, *ibid.*, 181-216, 235-272, *esp.* 216.

[27]Katz, *op. cit.*, 185-197.

[28]A. Tschermak, Ueber Kontrast und Irradiation, *Ergebnisse d. Physiol.*, II., 2, 1903, 726-745, *esp.* 765; Kroh, *op. cit.*, 255-261.

[29]Katz, *op. cit.*, 221-228, 354-358, 401-406; G. E. Müller, Ueber Jaenschs Zurückführung des Simultankontrastes auf zentrale Transformation, *Zsch. f. Psychol.*, 93, 1923, 1-16, *esp.* 10-11.

[30]This may be demonstrated as follows. A small red square (about 2 in.) is viewed against a large sheet of grey cardboard (24 x 36 in.). Under a non-critical attitude, without definite fixation, the whole sheet of cardboard immediately takes on a blue-green tone. If one then shifts to a critical attitude, with definite fixation, this *Flächenkontrast* disappears.

[31]Katz, *op. cit.*, 264-268.

[32]A. Gelb, Ueber den Wegfall der Wahrnehmung von Oberflächenfarben, *Psychol. Analysen hirnpathol. Fälle*, 1, 1920, 354; Die "Farbenkonstanz" der Sehdinge, *Handb. d. Normalen u. Pathol. Physiol.*, 12, 1929, 651-652.

[33]Katz, *op. cit.*, 282-290; E. R. Jaensch and E. A. Müller, Ueber die Wahrnehmung farbloser Helligkeiten und den Helligkeitskontrast, *Zsch. f. Psychol.*, 83, 1919-1920, 266-341, *esp.* 328-330.

[34]Katz, *op. cit.*, 406.

[35]Kroh, *op. cit.*, 269.

[36]Katz, *op. cit.*, 264-268; Kroh, *op. cit.*, 261-266; G. E. Müller, *op. cit.* 12.

well as 'transformation.'[37] The experiment reported by Koffka, which Kroh quotes as evidence that objectivity decreases contrast,[38] actually has no bearing upon the problem. Koffka presents the experiment as an example of the dependence of contrast upon 'mental' factors.[39] It shows very nicely that the tendency toward *Prägnanz* is sometimes stronger than the contrast tendency, but does not prove that objectivity decreases contrast.

(7) *In transformation, the greater the distance between the observer and the color the less the effect, but in contrast the greater the distance the greater the effect.*[40] Katz' assertion that 'transformation' decreases with the distance is based upon his work with achromatic colors under 'tone-free' illumination. Until these results are checked under other 'transformation' conditions, as with colored shadows, they hardly constitute proof of essential difference.

(8) *Transformation does not affect the differential limen, while contrast does.*[41] Here again, the evidence is conflicting. Koffka and Mintz find that 'transformation' does affect the D.L.[42]

(9) *In transformation, the longer the exposure time the better the effect, while in contrast the effect is immediate.*[43] Both Dr. Helson and the author have found that colored glows under colored illumination show the maximum 'transformation' effect the moment that the colored illumination is turned on.[44] These experiments should be repeated with Katz' more refined technique.

(10) *Transformation does not affect negative after-images, but contrast does.*[45] This argument refers, I suppose, to *Randkontrast*. At least, I find no evidence that *Flächenkontrast* differs from transformation in this respect.

Thus, when both sides of the argument are taken into account, the case for an essential difference between contrast and 'transformation' is by no means clear. Indeed, certain eminent psychologists favor discarding the 'transformation' concept entirely. Gelb says, in substance, that the whole problem centers about the *Aufbau* and *Struktur* of the visual world rather than about the distinction between 'retinal' and 'transformed' colors, which the 'transformation' concept implies. Phenomenal organization and configuration of the visual field explain all phenomena of color constancy. So, Gelb concludes, "*Es gibt nicht irgendeine bestimmte Beleuchtung, auf die allein man die Voraussetzungen und Ableitungen von Jaensch anwenden könnte: für jede Beleuchtung besteht dafür das gleiche Recht. Damit aber verliert der Transformationsbegriff theoretisch jede Bedeutung: er erscheint überflüssig.*"[46] Köhler has expressed similar opinions.[47]

[37]A letter of the alphabet (such as *S*), cut from gray paper and mounted upon a colored background under tissue paper, gives a greater contrast effect than a simple gray strip.

[38]Kroh, *op. cit.*, 266.

[39]K. Koffka, Zur Grundlegung der Wahrnehmungspsychologie, eine Auseinandersetzung mit V. Benussi, *Zsch. f. Psychol.*, 73, 1915, 39-40.

[40]Katz, *op. cit.*, 171.

[41]Katz, *op. cit.*, 138-142, 404-406.

[42]K. Koffka and A. Mintz, On the influence of transformation and contrast on colour- and brightness- thresholds, *Psychol. Forschung*, 14, 1930, 183-198.

[43]Tschermak, *op. cit.*, 748; Katz, *op. cit.*, 198-214; A. Kirschmann, Psychologische Optik, Abderhalden's *Handb. d. biolog. Arbeitsmethoden*, 6, 1927, 954.

[44]Helson, *op. cit.*, *Psychol Rev.*, 275; Wells, *supra*, 138.

[45]Katz, *op. cit.*, 408.

[46]Gelb, Die "Farbenkonstanz" der Sehdinge, 667-669.

[47]Köhler, *op. cit.*, 39-44.

Dr. Helson suggests that I have too easily disposed of the possibility that the cigarette phenomenon is a case of 'transformation.' Perhaps my brief reference to 'transformation' is partial justification for this inference. But obviously I would not have made the statement, "at present, the evidence for an essential difference between contrast and 'transformation' is inconclusive," unless I had considered that evidence most carefully.[48] It was such consideration that led me to define contrast so "loosely" as to include phenomena called 'transformation.' In so doing, I purposely placed the burden of proof upon those who would draw an essential difference between the two. Dr. Helson has apparently either overlooked or ignored this challenge.

Dr. Helson gives 8 'reasons' for ascribing the cigarette phenomenon to 'transformation' as opposed to contrast. 'Reasons' (1), (2), (4), (5), and (6) have been disposed of. His (3) harks back to Hering's theory of contrast, which is inadequate not only to 'transformation' but to contrast. Surely Dr. Helson must know that some psychologists have seriously considered the possibility that perception of illumination is mediated by the rods and the perception of objects by cones.[49] If this hypothesis be correct, contiguous retinal areas *are* involved. As for his (7), contrast subsides with *Dauerbeobachtung* only when there is close visual fixation.[50] Under the usual conditions of 'transformation' experiments, there is no definite fixation point. His (8) betrays a confusion of physical intensity with psychological brightness. The seen brightness of the illumination is determined, not by its physical intensity alone, but by the background against which it is seen and by 'color constancy' or 'transformation.' Dr. Helson has failed to take into consideration the very concept which he is seeking to demonstrate.[51]

Dr. Helson seeks to support his 'transformation theory' negatively by bringing numerous criticisms against my argument for the contrast concept. To anyone who has followed the discussion, it must be evident that these criticisms involve misinterpretations of my previous reports. Dr. Helson claims, for example, that his results were obtained under conditions highly unfavorable to contrast, although I have previously demonstrated that the conditions were excellent for contrast. Film and film-like colors, moreover, give better contrast effects than surface colors. Again, Dr. Helson objects to my definition of contrast upon the grounds that, "All writers on contrast regard as essential conditions for the phenomenon that *neighboring* fields shall be differently stimulated."[52] But, in my definition, I used the equivalent term *surroundings* and stated specifically that the surrounding area must be *differently stimulated*.[53] The *benachbarte Elemente* may be side by side in bidimensional space or *Nebeneinander* in tridimensional space. Dr. Helson speaks of my "choice of green as the color to be

[48]Wells, *Supra*, 686.

[49]Bühler, *op. cit.*, 127; S. Krauss, Stäbchenfunktion und Farbenkonstanz, *Zsch. f. Sinnesphysiol.*, 57, 1926, 262-000; E. Brunswik and L. Kardos, Das Duplizitätsprinzip in der Theorie der Farbenwahrnehmung, *Zsch. f. Psychol.*, 111, 1929, 307-320.

[50]B. Striegler, Die Abhängigkeit des simultanen Helligkeitskontrastes von der Zeitdauer der Induktion, *Archiv f. d. ges. Psychol.*, 79, 1931, 273-324.

[51]As I previously reported, the effect is best when the background is of approximately the same brightness (*psychologically*) as that of the glowing tip. (*Supra*, 139).

[52]Helson, *supra*, 693. [53]Wells, *supra*, 686-687.

mixed with the contrast color."[54] I did not *choose* green. As previously explained, if the red of the cigarette glow is cancelled by contrast-aroused blue-green, then the green of the cigarette predominates.[55] If, however, the red is incompletely cancelled, then the resultant color is a function of the remaining red and the green. Dr. Helson objects that the "beautiful green" of the cigarette tip is not accounted for by contrast because, when similar hues are involved, the net effect should be gray or the same color at a low degree of saturation.[56] But Helmholtz, one of Dr. Helson's authorities on this point, reports a similar case, in which an orange *Infeld* surrounded by a red *Umfeld* becomes green.[57] Dr. Helson's statement that "the complementaries of these indistinguishable (red) hues must all lie at one point,"[58] is contrary, not only to the evidence previously cited, but to Troland's tables.[59]

To sum up, neither Dr. Helson nor Mr. Newman has brought crucial evidence against my view that the cigarette phenomenon is a case of contrast. Dr. Helson's objections to the contrast theory rest solely upon his own misconceptions and a disagreement regarding the facts of color change. This disagreement can be settled only by further experimentation under carefully controlled conditions. On the other hand, Dr. Helson's ascription of the phenomenon to the category of 'transformation' involves the as yet unjustified assumption that 'transformation' is essentially different from contrast. Although I do not claim that the two are identical, I regard the evidence for essential difference as inconclusive. One must suspend judgment. For the present, then, we may conclude with Gelb that the 'transformation' concept appears *"überflüssig."*

Smith College E. Frances Wells

A Frequent Error Concerning Ebbinghaus' Experiments on Obliviscence

The discrepancy between the results of Ebbinghaus and those of later investigators concerning the rate of forgetting has frequently been explained as being due to overlearning. Ebbinghaus—so it is said—learned his lists of nonsense syllables to *one* correct recitation, while in the later investigations the subjects learned their lists to *two* correct recitations. This explanation, which as a matter of fact is based on an error, was first given in 1911 by Ladd and Woodworth[1] to account for the difference between the results of Ebbinghaus and Radossawljewitsch.[2] Since 1911, Thorndike,[3] Freeman,[4] Gates,[5] Dashiell,[6] Sandiford,[7]

[54]Helson, *supra*, 697. [55]Wells, *supra*, 136-137, 691. [56]Helson, *supra*, 695.
[57]H. Helmholtz, *Physiol. Optics*, Eng. trans. II, 1924, 267.
[58]Helson, *supra*, 698.
[59]L. T. Troland, Report of committee on colorimetry for 1920-21, *J. Opt. Soc. Amer. & Rev. Sci. Instru.*, 6, 1922, 553-554.
[1]G. T. Ladd and R. S. Woodworth, *Elements of Physiological Psychology*, 1911, 572-576.
[2]P. R. Radossawljewitsch, *Das Behalten und Vergessen bei Kindern und Erwachsenen nach experimentellen Untersuchungen*, 1907, 1-197.
[3]F. L. Thorndike, *Educational Psychology*, II, 1913, 305-307; *Educational Psychology, Briefer Course*, 1914, 245 ff., 252.
[4]F. N. Freeman, *How Children Learn*, 1917, 195 ff.
[5]A. I. Gates, *Psychology for Students of Education*, 1923, 260 f.: revised ed., 1930, 310 ff.; *Elementary Psychology*, 1925, 331 f.; revised ed., 1928, 329 f.
[6]J. F. Dashiell, *Fundamentals of Objective Psychology*, 1928, 363.
[7]P. Sandiford, *Educational Psychology*, 1928, 250.

and Monroe, De Voss, and Reagan[8] have given the same explanation for the discrepant results of these two investigators without placing the responsibility for the explanation upon Ladd and Woodworth.[9]

The cause of the discrepancy—whatever it may be—is not a difference in the learning criterion employed; for Ebbinghaus' criterion was *two* correct recitations—the same as Radossawljewitsch's—and not *one*.

Ebbinghaus' experiments, it will be recalled, took place at two different periods: 1879-1880, and 1883-1884. Regarding these two groups of experiments Ebbinghaus says: "Freilich sind dieselben nicht vollkommen vergleichbar. Bei den Versuchen der ersten Periode war nämlich, um das oben erwähnte flüchtige Erhaschen der Reihen in Momenten besonderer Koncentration einzuschränken, die Bestimmung getroffen, dass die Reihen gelernt würden, bis *zwei* fehlerfreie Reproduktionen nach einander möglich seien. In der späteren Zeit habe ich diese Bestimmung, die ihren Zweck doch nur unvollkommen erreicht wieder fallen gelassen und an der ersten glatten Reproduktion festgehalten."[10]

In reporting his studies on retention and obliviscence Ebbinghaus states that "die bezüglichen Untersuchungen fallen in die Jahre 1879–80;" and apparently wishing to remind his readers of the criterion he used during that period, he adds the statement that "das Lernen wurde fortgesetzt, bis ein zweimaliges fehlerfreies Hersagen der betreffenden Reihe möglich war."[11]

Ebbinghaus, moreover, sought to determine, in a corollary experiment, the effect upon retention of using one or two correct recitations[12] as the criterion of learning. He found that he retained after 24 hours practically the same amount whether he used the criterion of one correct recitation or two.[13]

It is not reprehensible to make an error—let him who has never made an error cast the first stone—but it is a pity that an error, particularly one having its origin in a secondary source, should be perpetuated.

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KARL M. DALLENBACH

ALBERT PAUL WEISS: 1879–1931

Albert Paul Weiss, Professor of Experimental Psychology at Ohio State University, died on April 3, 1931, after a heart illness which had confined him to his bed for the greater part of three years. Dr. Weiss was born in Steingrund, Silesia, on September 15, 1879, and was brought from Germany to America during infancy. Both his undergraduate and graduate courses were completed at the University of Missouri where he received the Ph.D. degree in 1916. With the

[8]W. S. Monroe, J. C. DeVoss, and G. W. Reagan, *Educational Psychology*, 1930, 181 f.

[9]Though Starch and Pintner do not discuss the discrepancy between Ebbinghaus' and Radossawljewitsch's results, they both mistakenly cite Ebbinghaus' criterion as one correct recitation. (Daniel Starch, *Educational Psychology*, 1919, 157; revised ed., 1927, 171; R. Pintner, *Educational Psychology*, 1929, 254.)

[10]H. Ebbinghaus, *Über das Gedächtnis*, 1885, 45 f.

[11]*Ibid.*, 91.

[12]Some authors who correctly report that Ebbinghaus used two correct recitations as his criterion of learning neglect to state that it was two successive recitations (zwei fehlerfreie Reproduktionen nach einander).

[13]Ebbinghaus, *op. cit.*, 109.

exception of his alma mater, Ohio State University was his sole academic connection. He was appointed instructor in 1912, assistant professor in 1916, and professor in 1918. He held office as president of the Ohio Academy of Science in 1922, as president of the Midwestern Psychological Association in 1929, and at the time of his death was a member of the council of directors of the American Psychological Association and chairman of the committee on the psychology of the highway appointed by the National Research Council.

A bibliography of Weiss' published work contains somewhat over forty articles and one book. The book and more than half the papers can be roughly assigned to the field of theoretical psychology. Prior to the appearance of his earliest writing in this field Weiss brought out several papers dealing with educational topics and also a group of articles and a monograph dealing with apparatus and experiments in the psychology of sensory processes, especially audition. Weiss never lost interest in these or any other problems upon which he had once worked. Even when his own pen was busiest with theoretical writing he devoted so much time to stimulating and directing the work of graduate students in a wide range of experimental investigations that it is a fair (behavioristic) assumption that he ranked his twenty-five Doctors of Philosophy as his greatest contribution to science.

The number of Weiss' students steadily increased, even during the years when his classes and seminars had to be held in his home. There, as before, his graduate students could obtain his characteristically rugged criticism of research plans or avail themselves of his ingenuity in devising apparatus indispe sable to their research—for Weiss was alert to prevent the multiplication or nrefinement of mechanical devices from becoming an end in itself. Although a constant stimulus to other workers he was never concerned over the meticulous ascription of credit for joint accomplishment. In 1929 as president of the Midwestern Psychological Association he delivered an address on the measurement of infant behavior, which forecast the findings and developed the theoretical implications of an important coöperative research attack upon the behavior of the infant in the first ten days of life, carried out under his leadership. The published reports of the study give less than due prominence to Weiss' name because his generosity and modest selflessness forbade his accepting full credit.

In 1925 Weiss published *A Theoretical Basis of Human Behavior*. Judging from the intrinsic nature of the book and also from the fact that he incorporated into it the gist of most of his theoretical papers and that he brought out a revised and enlarged edition in 1929, there appears to be even more than the usual justification in the case of a one-book writer for considering it as his *opus magnum*. It is a resolute exposition of an uncompromising behaviorism. Weiss was Max Meyer's pupil and assistant at the University of Missouri, and here is the full fruit of the inspiration Meyer had been to him, fittingly dedicated to the teacher.

No behaviorist could ever be more zealous or thorough-going than Weiss in eliminating subjective categories from psychology while holding to a primal faith in the competence of mathematics and scientific method to record and define a monism of physical substance. "Behaviorism claims to render a *more* complete and a *more* scientific account of the totality of human achievement *without* the conception of consciousness, than traditional psychology is able to render *with* it. The factors which traditional psychology vaguely classifies as conscious or mental

elements merely *vanish* without a remainder into the biological and social components of the behavioristic analysis."[1]

Weiss, like his teacher, was a physiologically and sociologically minded philosopher. Meyer might have been speaking for them both, and unintentionally testifying to his influence upon his most outstanding student, when he wrote: "A hundred years ago Johannes Müller, the father of modern physiology, made the famous remark: 'Nemo psychologus nisi physiologus'. . . . . Today a still more valuable statement would be this: 'Nemo psychologus nisi sociologus.' "[2]

Weiss used both his physiology and his sociology to serve the same end, to exorcize the psychic fiend from the science of behavior. More original and ingenious than his rather orthodoxly behavioristic version of physiological explanation and reduction without residue to biophysical and electron-proton correlates is his discussion of what he calls the biosocial responses, especially, language. He succeeds in showing how largely the reluctance to abandon animistic and mentalistic terminology springs from a failure to understand the rôle and significance of biosocial responses, and that language rightly understood can explain away the reputedly non-material ingredients of man. Biosocial responses are distinguishable from "biophysical" responses in that they function as socialized stimuli for evoking the responses of other individuals. Through them the separateness of individual nervous systems is bridged, human achievement in all its variety—vocational, professional, recreational and personal—is made possible and social organization is effected. In particular the adoption of conventional speech sounds and allied language forms, including mathematics, results in a kind of "sensori-motor interchangeability" between individuals whereby the individual nervous system is put in touch with a greater and greater number of objects and events past, future, and spatially removed from it. Social evolution is characterized by increased variability between human individuals and "the reduction of all objects and processes of the universe to library dimensions." A hypothetical superman is depicted whose diversity and endowments so far exceed ours, though in degree only, that he attains to omnipresence, omniscience and omnipotence, an individual whose solipsistic fate might be said to be more endurable than our own because he is "an aggregate of electron-proton systems whose changes exhibit the maximum degree of correlation with the changes that are occurring or have occured in all the other electron-proton systems in the universe."[3]

If we should venture to anticipate the riper judgment of the future it would be to say that Weiss' influence upon contemporary psychology through the work of his students, his associates at Ohio State University, and his wider circle of colleagues elsewhere probably counts for more than the effect of all he ever wrote. Weiss could point out to others the way to the heart of many a problem for which he himself, though a tireless worker, had no time to spare. He inspired a great deal of research. Though the tone of his systematic writing was inflexible and dogmatic, in the practice of science his insistence was not upon submissiveness but solely upon thorough and exact scholarship. In discussion his words disclosed the breadth and openness of mind characteristic of the experimental scientist.

University of Minnesota RICHARD M. ELLIOTT

[1] A. P. Weiss, *A Theoretical Basis of Human Behavior*, 2nd ed., 1929, p. vii.
[2] M. F. Meyer, *Psychology of the Other-One*, 2nd ed., 1922, 405.
[3] Weiss, *op. cit.*, 107.

BOOK REVIEWS

Edited by JOSEPH PETERSON, Peabody College

Human Speech: Some Observations, Experiments, and Conclusions as to the Nature, Origin, Purpose and Possible Improvement of Human Speech. By SIR RICHARD PAGET. London, Kegan Paul, Trench, Trubner & Co., 1930. Pp. xiv, 360.

This volume is included in the International Library of Psychology, Philosophy, and Scientific Method, and is written by a well-known English physicist who shows refreshing independence in his thinking and methods of experimentation. Unfortunately there are weaknesses in his interesting contributions to a work that is so largely anthropological and psychological, weaknesses which could have been avoided by collaboration with a well trained psychologist and a competent anthropologist. This does not mean that there are not important contributions to psychology. Paget's general thesis is that human speech has largely originated from 'gestures' of the organs of articulation. Man was driven "to the invention of speech," not so much by the "need of expressing his thoughts (for that might have been done quite satisfactorily by bodily gestures) as the difficulty of talking with his hands full." Difficulties arose in the case of such gestures when the hands had to be continually used in craftsmanship, in the chase, and in the beginnings of art and agriculture, and early man was driven "to find other methods of expressing his ideas—namely, by a specialized pantomime of the tongue and lips." Even the eyes would be so much occupied in the development of arts and crafts that they would be less available for recognizing gestures. "The apes, having (in their environment) no need of craftsmanship, thus escaped the necessity of a vocal language. As man developed in intelligence and technique, the need for more exact gestures became greater, while his hands became more occupied with the arts and crafts" (p. 132). This position is justified by reference to Darwin's observations of "serviceable associated habits," which he attributed to "imitation or some kind of sympathy;" *e.g.*, the moving of the jaws simultaneously with such hand activities as the cutting of something with a pair of scissors. "Then the great discovery was made that if while making a gesture with tongue and lips, air was blown through the oral or nasal cavities, the gesture became audible as a whispered speech sound. If, while pantomiming with tongue, lips and jaw our ancestors sang, roared or grunted—in order to draw attention to what they were doing—a still louder and more remarkable effect was produced, namely, what we call voiced speech." The ear thus came to take the leading part in recognizing the mouth pantomime or gestures "so that, for the first time, mankind was able to impart information in the dark, or when he was out of sight of the person with whom he wished to communicate" (p. 133). Surely no one well acquainted with modern biological psychology could write seriously such statements as those which I have quoted. Were infants silent until some genius had thus accidentally discovered the convenience of the use

of vocal sounds? And was the purposely adopted practice of vocalizations transmitted in heredity so that our infants are now born crying? Were emotional reactions silent before the lucky day of that 'great discovery'? Since lateral movements of the tongue or jaw produced little or no audible change, the vocal gesture movements, it is pointed out, were limited to two dimensions (up and down and front and back); and from these movements "nearly all human speech took its origin" (p. 134).

In the earlier part of the book the vocal organs are considered in relation to speech. Data are presented from experiments on vowel sounds which show that loud but not shouted voiced sounds carry from 8 to 19 times as far as unvoiced or whispered sounds made as loud as possible. These are, of course, only very rough measurements, but they illustrate the effectiveness of the vocal cords. Moreover, the involvement of the vocal cords adds a great variety of flexion, melody, emotional quality, and song. The author at first found the unvoiced vocal resonance effects very hard to recognize as musical sounds; but practice enabled him to recognize double resonances, and so to chart these vowels on a pitch-scale of semitones from 304 to 2579 d.v. that their upper and lower resonances gave two continuous series with the range of variation in pitch at different trials indicated by heavy lines, two for each vowel. Of these vowel sounds the lower ones in sounds pronounced as in *hat*, *earth*, *not*, and the upper ones in *no* and *who* extended over the widest pitch-range. These were all located in the range from $g^{1\#}$ to $a^{2\#}$ (406-912 d.v.). It was found that both the higher and the lower resonances would be made audible when sounds outside the mouth were produced by clapping the hands, and also that one note of either series could be held constant while the other was varied through a scale. Both notes could even be played in contrary motion—one series playing up while the other played down. No simple harmonic relation between the double resonances exists. The ranges of many of these resonances of different vowels overlap considerably, some being almost identical; but in such cases the other resonances of the two vowels are always well separated (p. 42). The idea of double resonance of vowels is, of course, not new. Paget suggests that one difficulty of recognizing the whistled octave-notes may lie in the fact "that they are made by two different organs of naturally different ranges of pitch and that in many cases at least, the observer cannot actually produce the same note by both methods" (p. 48).

When the vocal cords are thrown into action, more complex effects are produced. Some of these effects are examined. Extensive experiments on the production of these several vowel sounds with artificial vocal cavities—pipes, rubber tubes, and particularly plasticine models—are described. These experiments support the double-resonance view, contrary to the results obtained by D. C. Miller and Stewart with more refined methods, but in agreement with the findings, in 1924, of Crandall and Socia, who recorded electric waves by photographic methods. The author admits that certain discrepancies yet constitute unsolved problems regarding the physical basis of vowel sounds. His own contributions cannot be appreciated without a careful reading of the book.

The consonants likewise come in for critical observation and experimentation. These also Paget finds to be not mere noises but musical resonances of more or less sudden appearance, producible by physical resonators which are carefully described, so that his experiments may be repeated by other investigators. Con-

sonants (voiced, unvoiced, and nasal) are classified according to their mode of production, into front, middle and back. Each of these divisions is again classified as either full-closure and release or partial-closure. In the full-closure and released voiced-consonants, for example, *p* and *b* are front; *t* and *d*, middle; and *k* and *g*, back consonants. *V*, *z*, etc.; *l* and *r* (untrilled); and *y* as in (yes), gutteral *r*, and *h*; are front, middle, and back partial-closure consonants, respectively. The resonances of consonants, it is concluded, in so far as they are produced by movements of the vocal cords (like the diphthongs), "are characterized, not only by pitch, but also by their change and rate of change in pitch" (124 f.). The resonant ranges of consonants are as extensive as those of the vowels, but in certain of these resonances (there are always more than two in action simultaneously in the case of the consonants) the actual pitches produced depend mainly on the *front* resonance of the vowel with which the consonant is associated.

In later chapters descriptions and illustrations of tongue, mouth, and lip positions for the different vowels and consonants are given, and attempts are made to trace certain basic or root meanings in fundamental gestures both in young children's words and in those of certain widely different languages. In the case of the children's words, some of the illustrations given to support the theory seem to the reviewer far-fetched. The author, not being acquainted with child psychology, fails to recognize sufficiently in the child's language development the extent to which the social environment selects and emphasizes, from the child's early babblings and more or less accidental vocal sounds, those which have significance in the social milieu. There is, of course, good ground for saying that vocalizations made with the organs of speech in certain positions which are serviceable for eating, for tasting on different parts of the tongue, for 'gesturing,' etc. will have their pitches, inflections, vowel and consonant resonances considerably determined thereby; but a variety of other factors also operate, particularly when speech development takes place more or less reflectively. Even though the author is not a philologist, he has laboriously canvassed various sources of information on symbolism, roots, etc. in different, rather unrelated, languages. Some of these languages, he shows, could be greatly improved as to differentiation in meanings if more consonants were intelligently distributed among the vowels employed.

An interesting and suggestive chapter from the standpoint of research problems and practical applications ends the text of the book. There are, however, eight valuable appendices as follows: a mathematical analysis of the dual character of vowel sounds (by W. E. Benton); an audiogram of the author's hearing as measured in the Bell Telephone Laboratory, New York; tables of the dimensions, vowels, pitch, etc. of certain Paget and Clay variable vowel models; experiments with tubular vowel models; tabular data on consonant resonances; comparative data on American and English vowels; and a reprint of an article by J. Rae in *The Polynesian*, in which Mr. Rae approaches the author's theory.

J. P.

Reasoning in White Rats. Comparative Psychology Monographs, Vol. 6, No. 29, By NORMAN R. F. MAIER. Baltimore, The Johns Hopkins Press, 1929. Pp. 93.

In this study the general problem is, "Can a rat solve problems without previously having learned to respond in the situation concerned and without making

a series of random responses that finally lead to an adjustment?" In well planned experiments the rats, separated from food by a wire screen on the starting table, reached the food by a round-about route leading away from the table, the route having blinds and other obstacles. In several set-ups the rats first learned to reach the food by starting from some point along the course. After that they were released from the starting table, thus adding a segment to the pathway. The addition of easy sections resulted in a direct connection of both sections while complex additions brought out trial and error attacks. In other experiments the same rats learned to run from A to food at F over alternate pathways, X and Y. Then they were placed at F with the food at X or Y; one of the pathways had to be selected. On the third trial the rats were started from A, from which 85% of the first runs and 100% of the second runs were correct.

From such results it is concluded that rats can adjust themselves by intelligence and insight without trial and error. This is hardly convincing since, admittedly, trial and error was present in the early stages of every experiment, and the use of kinaesthetic, visual, and tactual clues was evidently not precluded. Furthermore, trial and error is evident in several situations wherein the rats learned only one of three completing segments leading to food. In such cases most of the rats upon being released from the starting table used the previously learned section in reaching the food in preference to taking two other equally accessible approaches.

The study undoubtedly shows that through training rats are capable of behavior slightly resembling reasoning, hitherto regarded as being possible only to higher animals. The implication of the study that rats know a familiar situation as an immediate whole, spatially, and not as a succession of experiences, may be important to the Gestalt theory.

University of Utah M. C. BARLOW

The Psychology of Religious Adjustment. By EDMUND S. CONKLIN. New York, Macmillan Co., 1929. Pp. xiv, 340.

The author looks at religion from the viewpoint of an empirical scientist, and consequently does not enter the domain of the philosophy or theology of religion. This attitude is maintained even in the relatively short space devoted to the examination of the various definitions of religion. He discards instinct and drives as inadequate definitions of religion. Emotion, attitudes, and beliefs, although prominently involved in religions, are individually not sufficient to identify religion. A broader definition is given, to include as religion "that behavior and those behavior products which are associated with a belief attitude toward some concept of a god or gods."

An examination of such behavior and behavior products, in both the individual and the group, forms the larger part of the book. The phenomena of religious adjustment, or maladjustment, are viewed on the same plane as the psychologist views any other psychological phenomena. Conversion, inspiration, and mysticism are found to be based on well-known principles of experience and behavior. The practices of organized religion are subjected to the same sort of scientific inspection.

The book has two limitations, naturally imposed by the scientific point of view. The student of philosophy will find little aid in understanding the meaning

of religion. Only a very limited view of religion is given. One gains the impression that religion means the typical religion of America to-day. The definition excludes all forms of religion which consist entirely of individual and social values, without reference to a supreme being. On the other hand, those who are looking for a key to their own religious adjustment will have to look still further. The middle ground between these two, the only one open to the scientist, is well, though not exhaustively, treated.

Duke University HOWARD EASLEY

Technik der Psychoanalyse. II. Die Analytische Reaction. By OTTO RANK. Leipzig and Wien, Franz Deuticke, 1929. Pp. 121.

The author of this little monograph, the second in a series of three, is obviously trying to clarify thought on psychoanalytic procedures. The details of technique he has here subordinated to the problem of comprehending the behavior of the patient in the course of analysis. He is dissatisfied with many of the time-worn concepts in the psychoanalytic conception of the psyche and the usage of them in the interpretation of what happens during an analysis; and he is at the same time seeking to simplify, as well as to harmonize with the obvious. Memories are important according to the way in which the patient reacts to them when they are revived. The patient's conception of himself and the world in which he lives, how he evaluates and why—these and other features are recognized as of first importance in the analytical reaction. Much of the cumbersome conceptual structure of psychoanalytical thinking is reduced to will and to will conflicts.

All this can but appeal to the psychologist as commendable, as steps in a direction long known to be necessary. But psychologists will regret the failure to utilize any of the available material on the psychology of will, choice, attitude, or emotion. The literature referred to, in addition to the author's own, is that of Freud and Adler and Jung. While these constructs of Rank may perhaps serve better than the earlier ones for therapeutic purposes, they constitute but a very slight contribution to the scientific interpretation of the behavior of a human being in the course of that sequence of experiences termed a psychoanalysis.

University of Oregon EDMUND S. CONKLIN

Modern Materialism and Emergent Evolution. By WILLIAM MCDOUGALL. New York, D. Van Nostrand Co., Inc., 1929. Pp. x, 249.

This book is really a supplement to *Body and Mind.* In these lectures McDougall seeks to show the causal efficacy of our mental or teleological activities. The outstanding characteristic of contemporary science is its denial of the reality of teleological causation. The question of the validity of modern materialism must be settled by an appeal to psychology. There are two orders of natural phenomena, the mechanistic and the teleological. Synthesis goes on in both the physical and the mental realms, but creative synthesis, or genuine emergence, is peculiar to the mental realm. There is no evolution in the physical world in the valid or significant sense of this term. Mind is the teleological factor in evolution.

Teleological causation is not "crypto-mechanistic." Memory is essential to evolution. McDougall accords high praise to the theory of memory advanced by Rignano, but even notable theory fails utterly to devise a physical basis for memory in the material structure of the brain.

These lectures are in a semi-popular form, having apparently been delivered before the Louisville Presbyterian Seminary. The professional psychologist, whatever his particular persuasion may be, can not, however, fail to give consideration to the contents of this little volume, representing as they do in a summary manner McDougall's views.

Hamilton College PAUL C. SQUIRES

Untersuchungen über Grundfragen der Akustik und Tonpsychologie. By E. R. JAENSCH and assistants. Leipzig, J. A. Barth, 1929. Pp. xviii, 172.

The several parts of this monograph have appeared earlier in the *Zeitschrift für Psychologie* and in the *Bericht über de Kongress für experimentelle Psychologie, Göttingen,* 1914. The investigations reported are of less general significance than the principal title suggests. Jaensch and his students have been concerned almost entirely with the characteristics of human speech, particularly vowels. For the experimental work they developed a "light siren," with which they were able to produce various simple and complex wave-forms and to test certain theories of vowel quality.

In the first study, Jaensch deals mainly with the theoretical aspects of his experimental work. He gives a few samples of various wave forms, showing their effects on the vowels and the relation of pitch and noise to the vowels. The second paper, by Hans Lachmund, repeats the description of the "light siren" and then describes the results obtained by systematic variation of the form of the stimulus wave. He also tests Helmholtz' theory of vocal synthesis. The third section is a brief note by Lachmund on the dependence of "apparent intensity" upon "subjective localization" of the sound. The fourth reports further experiments directed principally at the theories of Helmholtz and Stumpf. Finally, Jaensch summarizes briefly the results of the foregoing work. Both the apparatus and the results described are of considerable importance to all investigators in the field of audition.

Hobart College FORREST LEE DIMMICK

Developed Lessons in Psychology. By E. M. BAILOR and H. MELTZER. New York, Harcourt, Brace & Co., 1929. Pp. 199.

This book represents an effort to guide both teacher and pupil into the essentials of general psychology by means of selected topics, references, questions and answers. The topics selected, *e.g.*, Biological Foundations of Behavior, Unlearned Modes of Behavior, Changing Human Nature and Behavior, How We Learn to Know, embody some of the essentials in the field of psychology; and the bibliography, while by no means exhaustive, is representative.

The young teacher should find the volume suggestive as a guide to a course in general psychology. The book, by its method of handling the topics, fails, however, to train the student in experimental technique. It is written, as are so many present day aids, from the point of view of drilling 'facts' into the student. The experienced teacher will find the outline of only mediocre assistance. For the student, the work presents a fine opportunity for short-cutting and directing his reading with respect to getting answers to isolated questions, merely to get

the answers, a procedure usually resulting in the memory of disconnected 'facts.' But that is not altogether the fault of the book, for its purpose is to aid one's reading and to stimulate thinking.

The authors propose only incidentally, by means of their questions and answers, to summarize the sources cited in the references.

Peabody College JESS H. EDDS

Psychologische Studien: Arbeiten über theoretische und praktische Fragen der Psychologie und ihrer Grenzgebiete. Jubilee volume in honor of Dr. Paul Ranschburg. Edited by JOHANN SCHNELL. Budapest, Royal Hungarian University Publication, 1929. Pp. viii, 493.

This volume is inscribed to the dean of Hungarian psychologists on the occasion of the twenty-fifth anniversary of the opening of the Royal Hungarian Clinical Psychological Laboratory, of which he is the founder and director. The tribute comes from his past and present pupils, and from foreign and Hungarian admirers and colleagues. Most of the articles are written by Hungarian psychologists and psychiatrists, but there are contributions also from Berlin, Munich, Giessen, Baden-Baden, Altona (Hamburg), Vienna, and Moscow. The volume is bi-lingual; an Hungarian abstract is appended to the German articles, and a German abstract to those written in Hungarian.

The range of articles testifies to the wide compass of Ranschburg's scientific activity. Experimental psychology, psychophysics, vocational psychology, child psychology, clinical psychology, psychopathology, neurology, endocrinology, neuro-anatomy, etc., are all represented.

Johann Schnell gives an interesting sketch of the scientific personality of Ranschburg, whom he describes as the founder of experimental psychology in Hungary. Born in 1870, Ranschburg has been scientifically active since 1894; he is both physician and psychologist; his first work was in the field of hypnotism and hysteria, gradually encompassing more and more of medical, physiological, and experimental psychology. In this last field his work on attention, association, and memory have attracted most interest. Ranschburg has received much deserved recognition from Hungarian and foreign psychologists. In 1928 he was elected president of the newly-constituted Hungarian Psychological Society.

August Ley of Brussels sends his greetings, recalling that Ranschburg was the first to study memory in connection with associated words, with due recognition of the affective value of these words, instead of working in the "dry and arid world of pure memory, with its tok, sul, bir, its tiresome numerals, and its abstract geometrical forms." Sante de Sanctis of Rome describes Ranschburg as one of the few medical psychologists who see the problem of aiding abnormal children from the scientific point of view, that is, as a problem in applied psychology.

American psychologists, even though they are not represented in this jubilee volume, will surely sympathize with and approve of this tribute to the work of a very distinguished psychologist.

Columbia University OTTO KLINEBERG

Applications of Psychology. By FRED A. MOSS. Boston, Houghton Mifflin Co., 1929. Pp. x, 477.

Moss divides his treatment into three major parts: four chapters on "Determining Forces in Human Behavior;" eight chapters on "Individual Differences;" and eight chapters on "The Applications of Psychology in Professional and Industrial Fields." The first section serves chiefly to present conclusions the author has drawn from his experiments upon the motivation of animals. Basing his statements upon such experimental work, he propounds twelve laws of motivation, all of which may be subsumed under the general law: "The behavior of any animal . . . is the resultant of his drives to action and the opposing resistances" (p. 18). Other chapters of the same section deal with the effects upon behavior of internal and external stimuli, such as fatigue, drugs, ventilation, and climate.

The second section offers as its text the statement that "individuals are neither born equal, live equal nor die equal" (p. 83). Here the author provides a conventional treatment of individual differences discussing in successive chapters the effects upon the individual of race, family, experience, age, sex, and emotional outlet, with a final chapter on the measurement of personality. He attempts to avoid the nature *vs.* nurture controversy by taking the position that "heredity . . . determines *how far* the individual can go; the environment determines *in what direction* his progress will be" (p. 155).

The final portion of the book deals with the applications of psychology to medicine, to law and criminology, to advertising and salesmanship, to personnel work, to traffic regulation, to politics, and to education. In this, as in the earlier sections, Moss has furnished references to the experimental literature of the various fields. These references are, in general, well chosen, and are marred only by the lack of any attempt to appraise their scientific value. This same lack of critical treatment is much more marked in the author's use of folk-lore, newspaper clippings, and quotations from poems and songs to establish points throughout the volume, as well as in his tendency to make dogmatic generalizations without any effort at substantiation. Thus, in the section on individual differences, we learn with some amazement that "the white child has to learn to dance, but the little pickaninny apparently dances instinctively. With the negro, rhythm seems to be an innate quality" (p. 104). Just a few pages from carefully selected references to Galton's work, we find, without further comment, the statement that "whereas the mountaineer talks little and thinks much, the lowlander talks easily and ponders less" (p. 73). Such unguarded comments as these, together with a marked tendency to fall into the *post hoc propter hoc* fallacy in interpreting experimental results, are the outstanding weaknesses of a volume whose greatest virtue lies in its stimulating excursion into little-explored fields.

Cornell University JOHN G. JENKINS

Experimentell-vergleichende Untersuchungen über die sittliche Bildung in den verschiedenen Volksschulformen. By ARTHUR WELKISCH. Langensalza, Hermann Beyer & Sons, 1929. Pp. 93.

This monograph deals with the question of moral education in various types of German public schools. Three different schools are compared with regard to their effect upon the ethical insight of the pupils,—(a) a school with definite

religious and confessional education, (b) a school with definite ethical training along the lines of Christian morality, and (c) a school without any definite religious or moral training whatever. From these three schools children in the third, fifth, and seventh school years were chosen for study.

The subjects were asked (p. 20) about the duties which they recognized (*e.g.* what duties have you to carry out at school, at home, etc., and why do you accept them as duties?), their ideals (*e.g.* what sort of person would you like to be, and why?), their behavior in certain imaginary situations (*e.g.* a poor boy, whose sick mother is in need of food, finds a pocket-book containing two dollars; what should he do? What do you think would be right? Why?), and the actual behavior of the class under certain conditions (*e.g.* describe what happens in your classroom when as a result of your teacher's illness you are left alone a whole morning).

The results show that the children of the religious school recognize a larger number of duties than do those of the other schools and that they can formulate their ideals more clearly. On the other hand they name fear and force much more frequently when giving reasons for their actions; with them the strength of authority is all-important. There is no real feeling of confidence between teacher and pupils. The pupils admit that they would not do their work or keep still in the absence of their teacher. In the other two schools the pupils are less impressed by fear and external force, and seem rather to have achieved some insight into social realities and ethical autonomy. There is much more self-control on the part of the group, which can proceed with its work without the help of the teacher for a considerable period.

The author admits that a study of this kind is inconclusive as regards the real inner moral feeling of the pupils, but believes that it can give some information as to their moral insight and judgment (p. 78).

The whole investigation seems to the reviewer to be of doubtful value. There is an attempt to win objectivity by means of the quantitative method, but not very much is gained by the knowledge that in one school 3.8% of the children and in another 3.1% mentioned religious ideals. Even when the difference is large, its meaning is still obscure. The use of statistics in the study of these ethical imponderables must wait upon the discovery of a more objective and less arbitrary method of getting at the subject's real attitude towards ethical situations. There is a bibliography of thirty-six titles.

Columbia University OTTO KLINEBERG

Storia della Filosofia. I. Il Pensiero Antico: Storia della Filosofia Greco-Romano. By RODOLFO MONDOLFO. Milan, Albrighi, Segati & Co., 1929. Pp. iv, 515.

The usual history of philosophy consists of interpretative paraphrases and summaries of the works of philosophers in the attempt to reconstruct reflectively the course taken by thought in an assumed evolution. Ostensibly, the interpreting ideas are suggested by the material in hand, but at times at least, interpretation seems to have gone so far as to rearrange the apparently given sequence of systems and probably to force meanings upon them in the interest of finding in the historical sucession the illustration of a preconceived view of development. Such histories of philosophy are valuable, but they need to be checked by such parallel readings in the philosophers themselves as are furnished in collections

such as Diels' "Fragmente der Vorsokratiker," in which either original texts or translations are presented without specific historical organization. Besides this we have had as a unique example the *Historia philosophiae graecae* of Ritter and Preller which presents for a limited period the development of philosophy in the words of the original authors. In the present work Mondolfo has begun the first attempt to carry out this idea of Ritter more consistently and more systematically for the whole history of philosophy. Hand in hand with the Italian translation of the original texts, most of which are not easily accessible, there is given in smaller type and set off by brackets Mondolfo's own interpretation of the passages in question, which is balanced by a careful discussion of divergent readings and views. In the opinion of the reviewer, this critical adjunct is one of the most valuable features of this first installment of the work which closes with the Neoplatonic philosophy.

Vanderbilt University HERBERT SANBORN

General Psychology for College Students. By C. N. REXROAD. New York, Macmillan Co., 1929. Pp. xv, 392.

This is a general text-book written from the standpoint of a behavioristic psychology. The author is greatly indebted to Weiss and makes special acknowledgement of his influence. The first of the three parts of the book states the assumptions underlying the behavioristic point of view together with those biological and physiological concepts which "throw most light on the problem of human behavior." The concept of metabolic gradient, as developed by Child and others, is emphasized.

The second section of the book deals with the problems of maturation and learning. Unlearned responses are grouped on the basis of their complexity into reflexes, orientation responses, pattern responses, serial responses, and random activities. The conditioned reflex is accepted as the all-sufficient explanation of learning, which results in a neglect of the more complex and constructive types of learning. Part three is divided into two sections, the one discussing common accomplishments and the other individual differences.

The introduction to the field of psychology proper (ch. 2) is a statement, in the form of five postulates and their corollaries, of the assumptions underlying the behavioristic position. The reviewer feels that such statements should be the conclusion and logical outcome of a course and supported by experimental evidence rather than assumptions laid down *a priori* at the beginning. There is danger that much of the material that follows will appear as deductions from the principles rather than accurate descriptions and explanations of what really takes place.

The rather lengthy discussion of the nature of psychological explanation is the best that the reviewer has seen. Throughout the book, however, there is a tendency to simplify so as to state results in the form of laws, often at the expense of accuracy. Consider, for example, the statement that "Repeated stimulation by a particular stimulus makes it increasingly effective and others less effective" (p. 141). In contrast with other recent texts in general psychology, small space is devoted to presenting the results of experiments and relatively a large proportion is given over to general statements and conclusions. This is probably un-

fortunate. One is struck by the absence of theories, stated as such. The psychologist will find many explanations given in terms of current theories which are implicitly accepted without mention being made that they are not universally agreed upon. The author has certainly not stated all his assumptions. The reviewer doubts that this is the best method of developing a critical attitude on the part of beginning students. There is danger that a course conducted along the lines indicated by this text will result in indoctrination rather than the development of critical open-mindedness.

The style is clear and concise. Illustrations are good and well chosen. A bibliography, in which errors occasionally exist, follows every chapter; for example the three articles accredited to Perris [Perrin] and Klein were really written by Joseph Peterson. The omission of initials in the Index of Names occasionally gives one author credit for contributions by another. McDougall, Paterson, Uexküll, Cattell, Dashiell, are some names that are misspelled.

University of North Dakota C. W. TELFORD

The Psychological Approach to Reality. By FRANCIS AVELING. London, University of London Press, 1929. Pp. iii, 245.

Truly, psychology has come a long way in the last half century or less. What was once a rationalistic discipline among other philosophical disciplines has changed in that time to as different a field of thoughtful endeavor as could be imagined—an empirical biological science among other natural sciences. Any one who fails to appreciate the extent of the change and who wants a way of realizing it vividly, should read this book.

Armed with Spearman's three principles of noegenesis, Aveling addresses himself to that favorite problem of modern philosophy, How can we know Reality? Proceeding from a solipsistic standpoint of doubting (in something of the Cartesian manner), he inquires of his own immediate conscious experience as to the status of empirical reality, with its "apprehends," "percepts," etc., of ideal reality, with its "abstract elements," "educts," "constructs," and "correlates," of causation, based upon our notion of "efficient causality," of final causes and their character of "necessity," and even of transcendental reality which stands over against all the phenomenal reals just named. In spite of a few references to experimental work on will, such as that done in the Würzburg and Louvain laboratories, the procedure of the inquiry is on the whole not empirical (at least in the experimental, clinical, or statistical senses of contemporary psychology) but dialectical. The author disclaims any metaphysical interest and calls his whole problem "a purely psychological one"—in which case it seems that where proof of anything outside of conscious experience is impossible except in terms of conscious experience it is supposed to be a psychological matter.

The professional psychologist as such will find nothing in this book that he can use. On the other hand, if, when he doffs his laboratory apron, his speculative interests run high and epistemological doubts tend to awaken intuitional and dialectical efforts, then he will find an interesting line of argumentation put forth in this work.

University of North Carolina J. F. DASHIELL

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